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RESEARCH PLANNING CONFERENCE ON LANGUAGE DEVELOPMENT IN DISADVANTAGED CHILDREN

JUNE 7-8, 1966

Sponsored by

PROJECT ON RESEARCH DEVELOPMENT RELATED TO THE
DISADVANTAGED AND/OR SEGREGATED
EDMUND W. GORDON, CHAIRMAN
DEPARTMENT OF EDUCATIONAL PSYCHOLOGY AND GUIDANCE

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Introduction

The conference reported herein is one of a series focussing on issues related to the education of disadvantaged children and youth. The history and intent of this particular conference are discussed by Dr. Edmund Gordon in his opening statement which can be found beginning on page iii of these proceedings. It is our intention here to speak briefly about the organization of this document and about the organization of the conference it purports to represent.

The two-day conference consisted of 3 general sessions devoted to the presentation of six papers with comments and discussions following each. There were also two workshop sessions during each of which the conferees divided into 3 groups to discuss various issues raised at the general sessions. A final general session heard reports from the workshops and a summarization by Dr. Fishman. The degree to which the papers presented were delivered from written texts varied widely from speaker to speaker. Consequently the speeches were transcribed from tape recordings of the sessions and sent to the participants for addition or correction; because it is our conviction that English to be standard need not always be stiff, we have asked the participants whose speeches were delivered informally to refrain from translating them into formal English. They have kindly complied. The responses and the discussions following the speeches have not been edited by the participants, but by the editor, who assumes full responsibility for any misunderstandings which may result from infelicitous editing.

It has seemed to us that substance is often more meaningful and useful than sum. For this reason we have chosen to reproduce some of the discussions almost verbatim, removing only those non-substantive digressions and occasional repetitions in which scholars sometimes indulge. Because of this bias in favor of substance, we have also chosen a rather unconventional way of reporting the workshops. The workshop summaries as delivered at the conference quite rightly gave the impression that the issues dealt with were similar from workshop to workshop and that similar approaches to them were often taken. However, in looking over the available tapes of the workshops, it seemed to us that most of the participants had managed, (often in spite of their fellow discussants rather than with their cooperation) to "get their licks in". It seemed to us that it would be wasteful to let these always thoughtful and often useful statements simply be absorbed into general--and therefore much less suggestive-summaries. We have therefore omitted the summaries of the workshops as they were presented and in their place we have brought together under some very general headings, a series of statements made in the workshops which we felt should be a permanent part of the proceedings. Due to the fallibility of tape recorders, we did not have full transcriptions of all of the workshops. Some contributions are, unfortunately, irretrievably lost.

This conference was full of controversy. While it proved possible physically to bring together researchers from a number of disciplines and professional educators of varying points of view, it often seemed during these two days much less possible to stimulate a dialogue from which the children with whose language we are concerned might profit. For those of you who did not attend the conference, it is our hope that the proceedings will afford you not only information and insights but some indication of the passion with which frankly opposing points of view were offerd and defended. For those of you who participated in the conference it is our hope that you may discover as did those of us who have assumed the task of pulling the proceedings together for publication, that much of what seemed like heat alone at the time, appears in the absence of its paralinguistic surround to shed some considerable light on the issues with which we are all concerned.

Beryl Bailey, Ph.D.
Joan Gussow, A.B.
Vivan Horner, M.A.

"How hard one must work in order to acquire his language,--words by which to express himself! I have known a particular rush, for instance, for at least twenty years, but have ever been prevented from describing some (of) its peculiarities, because I did not know its name nor any one in the neighborhood who could tell me it. With the knowledge of the name comes a distincter recognition and knowledge of the thing. That shore is now more describable, and poetic even. My knowledge was cramped and confined before, and grew rusty because not used,-- for it could not be used. My knowledge now becomes communicable and grows by communication. I can now learn what others know about the same thing."

H. D. Thoreau, 1858

Thoreau, H. D. A Writer's Journal. (Laurence Stapleton: Ed), New York: Dover, 1960. pg. 184.

Colleagues and students, I want to thank you all for the enthusiasm with which you accepted our invitation to take part in this research planning conference on language development in disadvantaged children. I take it that this is a burning issue which each of us here present finds to be of utmost importance at this time, and that this is the reason that a conference of this kind has generated such tremendous response from those of you who are now present.

Since the purpose of this conference was to get a sort of cross-pollination, the sessions have been organized so that we will hear papers not necessarily related to each other. People from a number of different disciplines have been looking at this issue of language disadvantage, but we have all tended to go our own ways, not paying too much attention to what our colleagues were doing. Quite often, even within the confines of a single university, people will be working on the same problem who do not know of each other's existence and who certainly don't know, each of them, what the others are doing. At this conference we hope to acquaint each other with the kinds of research in which each of us is engaged. If we don't get them to know what's happening, we should at least look into ways of bridging the communication barriers.

I would now like to call upon Dr. Edmund Gordon, Director of the project under which this conference and others of its kind are being conducted, to give a word of welcome and to say something about this research program.

Beryl Bailey

Thank you Dr. Bailey and Good morning Ladies and Gentlemen. We are always pleased to have visitors come to New York and visit Yeshiva University, particularly those who have deep interests in some of the areas that are of primary concern to us. The project under which we are operating today is a project funded by the Office of Education, concerned with the stimulation of research activity related to problems of educating the disadvantaged. The precursor to this particular meeting was a small working conference last fall where a number of the participants in this conference came together for two days to look at some of the developments, issues and problems related to language development in disadvantaged children; and trying to specify these with greater clarity. It was our thought that such a conference might well be followed by a larger meeting, this time including participants who are in some respects junior persons in the field, that is those who are either still in training or just beginning their work as professionals in this general area. It was our hope that such persons could thereby be stimulated to move more actively into research activities related to language development in disadvantaged populations.

This particular conference has been organized primarily by Dr. Beryl Bailey whom I would like to thank now for her efforts in bringing this group together. There are some of us here who were supposed to help her, but our major contributions, I think, were those of criticism and discussion. The work was all hers. In the course of your two days here, it is our hope that, in addition to exposing you to some of the better work going on in this particular subject area, we can interest those who are not yet terribly active as researchers in this field in moving into it with greater speed and deliberateness, and can encourage those of you already working in the field to continue.

We happen to be in the very fortunate position these days of not being short of the material resources with which to foster these kinds of investigations. We are somewhat embarrassed by our riches, and by our relative lack of competent and interested researchers to pursue some of the primary issues related to the development of persons who may be called underdeveloped. If the conference can help to spur along those among you who are in a position to undertake quickly the study of such problems, we will feel that we have made a contribution. If, in addition, we can add to your understanding of issues in this field, if we can raise to a higher conceptual level your awareness of some of these problems, the conference, we think, will have served a useful purpose.

Over the past few years, I have become identified with and have expressed a continuing interest in stimulating and developing research and services for disadvantaged children. In some of my writing and many of my speeches, I have tended to discuss this problem in the context of children handicapped by their social status differences, and have, I think, too often disregarded the fact that large numbers of these youngsters are Negro or members of other minority groups. In New York, and in an institution like Yeshiva, it is easy for us, or at least for me, to forget the fact that racial discrimination is a major part of this problem and is ever present with us. But the realities of life do have a way of rather rudely intruding themselves upon all of us from time to time. They intruded on me, very personally, in a search for housing up in Massachusetts, where I was reminded that although I am a professor, I am also obviously a Negro, and discrimination in housing is certainly with us. And all of us were shocked yesterday by the reminder that racial hate, racial prejudice, is rampant in most sections of this country, if not all. The shooting of Mr. Meredith in Mississippi was a rather tragic event, and fortunate only in the sense that it could have been considerably worse.

These reminders serve to call our attention to the fact that as important as are our concerns with some of the more scholarly and technical aspects of education and development for disadvantaged persons, there is no question but that crucial to the status of such persons, to the development of such persons, and to opportunities for such persons, is the fact of minority group treatment in this country, the reality of racial discrimination, racial hate. It is interesting that at a time when our nation calls upon all of us to fall behind a mistaken national administration acting "in defense of freedom" in Vietnam, that we have not somehow managed to defend freedom in Mississippi, or in Massachusetts, or in New York.

I suspect that until we as a nation, and particularly we as individuals come to recognize that one does not defend human rights or freedoms abroad through the support of military dictatorships, through the use of napalm bombs, through the destruction of the homes of peoples, through the destruction of an undeveloped nation, like Vietnam, I suspect we as a people are not ready to defend freedom at home, nor to advance human rights at home because we have not learned that these rights are not a function of political beliefs or economic relationships, but that human rights are primary to all other things. Today, as we turn our attention to the scholarly endeavors that we have come here to pursue, I hope we can keep in mind the existence of questions that are broader and and, at least in the perspective of history, even more important than some of the crucial research problems that we will be considering. These are problems centering around the relations that exist between men. If we do not find ways of resolving these problems, our work can be destroyed, just as we can be destroyed. Racial hate, national hate, economic exploitation, these are the evils of our society, part of the fabric of our society out of which the status and the handicaps of disadvantaged children grow, and no matter how hard we work at the technical aspects of improving their lot, unless we as citizens can also work at the social and political aspects of improving their lot, at establishing democratic relations among people, and peace among nations, our research efforts are likely to come to little avail.

Edmund W. Gordon

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ON COMMUNICATIVE COMPETENCE

Dell Hymes

University of Pennsylvania

I want to thank Dr. Bailey and Dr. Gordon for inviting me to participate in this research planning conference on language development in disadvantaged children. My assignment from Dr. Bailey has been quite simply, and open-endedly, "a theoretical paper." One connotation of "theoretical," I am afraid, must be that I know too little about the actual subject to say something practical. Good practical work, however, must have an eye on the current state of theory; it can be guided or misguided, encouraged or discouraged, by one or another theoretical view. Moreover, the problems of language development in disadvantaged children have a particular pertinence just now for theory. The burden of my remarks will be that the practical problems and theoretical problems indeed here converge.

It is not that there exists a body of linguistic theory that practical research can merely apply. It is rather that work motivated by practical needs may help elicit and help build the theory that we need. Let me review the present state of linguistic theory in order to show why this is so.

Consider a recent statement:[1]

"Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance".

From the standpoint of the children we seek to understand and help, such a statement may seem to be almost a declaration of irrelevance. All the difficulties which confront the children and us seem to be swept from view.

One's response to such indications of the nature of linguistic theory might be what can be called "pick-and-choose." Useful models of language structure, after all, can be of benefit in ways not formally envisioned in the theoretical statements of their authors. Some linguists (e.g., Peter Rosenbaum, Lita Gleitman) are using transformational generative techniques to characterize ways in which some speaker-listeners in the same general speech-community differ from one another; moreover, some of these differences clearly involve imperfect knowledge of the language. Perhaps one's attitude, then, ought to be simply to disregard what linguists say about theory, as being primarily concerned with something not of primary concern to us. One can point to various models of language structure available to us--Trager-Smith-Joos, tagmemic, transformational-generative (in its MIT and Pennsylvania and other variants), stratificational; note that there are distinguished scholars actively involved with the use of each in the analysis of English; regret that linguists remain unable to agree on the analysis of English (let alone on attitudes towards schooling and children); and pick and choose, depending on problem and local situation, leaving grammarians otherwise to their own devices.

Only to "pick and choose" would be a mistake, however, for two reasons: the sort of linguistic theory quoted above, despite its narrowness, _is_ relevant in a special way that is important always to have in mind; and there is a body of linguistic problems and data that will be left without theoretical insight, if linguistic theory is left with such a narrow definition.

First, as to the special relevance of the view of linguistic theory cited above. Its representative anecdote (to use Kenneth Burke's term), the image it puts before our eyes, is that of a child, born with the ability to master any language with almost miraculous ease and speed; a child who is no mere passive object of conditioning and reinforcement, but who actively applies a truly cognitive skill to the unconscious theoretical interpretation of the speech that comes its way, so that in a few years and with a finite experience, it is master of an infinite ability, that of producing and understanding in principle any and all grammatical sentences of its language. When the image of the unfolding, mastering, fluent child is set beside the real children in many of our schools, the theoretical basis of the image is seen for what it is, _not a doctrine of irrelevance, but a doctrine of poignancy_. Such theory is based on the essential equality and potential of each child in his or her capacity simply as human being. It is noble in that it can inspire one with the belief that even the most dispiriting conditions can be transformed; and it is an indispensable weapon against views which would explain the communicative difficulties of groups of children as inherent, perhaps racial.

Second, as to the narrowness for our needs of the theoretical standpoint just described. It is, if I may say so, rather an Adam and Eve, a Garden of Eden standpoint. I do not think that the restriction of theory to an ideal speaker-listener is merely a simplifying assumption of the sort all scientific theories must make. If that were the case, then some explicit place for social complexities might be left, and no such place is defined. In particular, the concepts of linguistic competence and linguistic performance, as discussed in the work from which the quotation is taken, do not provide the theoretical scope that is required. _Linguistic competence_ is understood as exactly concerned with idealized knowledge of language structure--semantics, syntax, phonology. _Linguistic performance_ is understood as concerned with the modifications introduced by the processes that have often been termed encoding and decoding. Some aspects of performance have a constructive role to play, e.g., cycling rules that help assign stress properly; but if the passage quoted above is recalled, and if the examples of performance phenomena in the chapter quoted are reviewed, it will be noticed that the note struck is one of limitation. I do not think this note of limitation to be accidental. Rather, I take the motivational core of the theoretical stance to be one which sees linguistic competence as an idealized Garden of Eden sort of power, and the exigencies of performance as rather like the eating of the apple of the Tree of Knowledge, thrusting the one perfect speaker-hearer out into a fallen world. But of this fallen world, where meanings must be won by the sweat of the brow, and recreated in labor, almost nothing at all is said. The image is of an abstract and isolated individual, not, except contingently, of a person in a social world.

I take such limitations to disclose an ideological aspect to the theoretical standpoint in question. The theoretical stance of any group should always be examined in terms of the interests and needs unconsciously served. Now a major characteristic of modern linguistic theory has been that it takes structure as primary end in itself, and tends to depreciate use, while not relinquishing any of its claim to the great significance that is attached to language. (Contrast classical antiquity, where structure was a means to use, and the grammarian subordinate to the rhetor). The result can sometimes seem a very happy one. On the one hand, by narrowing concern to independently and readily structurable data, one can enjoy the prestige of an advanced science; on the other hand, despite ignoring the social dimensions of use, one retains the prestige of dealing with something fundamental to human life.

In this light Chomsky is quite correct when he writes that his conception of the concern of linguistic theory seems to have been also the position of the founders of modern general linguistics. Certainly if modern structural linguistics is meant, then a major thrust of it has been to define the subject matter of linguistic theory in terms of what it is not. In de Saussure's linguistics, as generally interpreted, la langue was the privileged ground of structure, and la parole the residual realm of variation (among other things).[2] Chomsky associates his conceptions of competence and performance with the Saussuring conceptions of langue and parole, but sees his own conceptions as superior, going beyond the conception of language as a systematic inventory of items to renewal of the Humboldtian conception of underlying processes. The Chomsky conception is superior, not only in this respect, but also in the very terminology it introduces to mark the difference. "Competence" and "performance" much more readily suggest concrete persons, situations, and actions. Indeed, from the standpoint of the classical tradition in structural linguistics, Chomsky's theoretical standpoint is at once its revitalization and its culmination. It carries to its perfection the desire to deal in practice only with what is internal to language, yet to find in that internality what in theory is of the widest, or deepest, human significance. No modern linguistic theory has spoken more profoundly of either the internal structure or the intrinsic human significance.

This revitalization flowers while around it emerge the sprouts of a conception that before the end of the century may succeed it. If such a succession occurs, it will be because, just as the transformational theory could absorb its predecessors and handle structural relationships beyond their grasp, so new relationships, relationships with an ineradicable social component, will become salient that will require a broader theory to absorb and handle them. I shall return to this historical conjecture at the end of my talk. Let me now develop some of the particular sorts of data which motivate development of a broader theory. And let me do this by first putting forward some alternative representative anecdotes.

As against the ideal speaker-listener, consider Bloomfield's account of one Menomini he knew:[3]

"White-Thunder, a man around forty, speaks less English than Menomini, and that is a strong indictment, for his Menomini is atrocious. His vocabulary is small; his inflections are often barbarous; he constructs sentences of a few threadbare models. He may be said to speak no language tolerably. His case is not uncommon among younger men, even when they speak but little English".

Bloomfield goes on to suggest of the commonness of the case that "Perhaps it is due, in some indirect way, to the impact of the conquering language." Social factors are suggested to have entered here not merely into outward performance, but into the inner competence itself. And the one thing that is clear in studies of subcultural differences in language development is put by Courtney Cazden in her excellent review article as follows:[4]

"The findings can be quickly summarized: on all the measures, in all the studies, the upper socio-economic status children, however defined, are more advanced than the lower socio-economic status children."

The point of course is not that social factors enter only to interfere. The differences just summarized involve positive social factors on the one side as much as negative ones on the other. It may indeed be the case that some or many lower socio-economic status children excel in aspects of verbal skill not observed or measured in the tests reported.

The generic role of social factors has been stressed by Labov, reporting on information as to ability to perceive phonological contrasts:[5]

"The contention that native speakers can hear phonemic distinctions much better than nonphonemic distinctions was not borne out by the evidence. Instead, one might say that the ability to perceive distinctions is determined largely by the social significance of the distinction to the listener."

Here are recurrently found differences within one and the same speech-community, entering again into the inner competence itself. It seems clear that work with disadvantaged children needs a theory of competence that can take account of socially conditioned differences in a natural and revealing way.

What would such a theory be like? No one knows better than those here today that very little of the content of such a theory can now be specified. Permit me, however, to take up again the representative anecdote of the child in order to sketch briefly what a broad (as distinct from a narrow), or perhaps a strong (as distinct from a weak) theory of linguistic competence would entail. Recall that in terms of the narrow theory one is concerned to explain how a child can come to produce and understand (in principle) any and all grammatical sentences. Consider a child with just that ability; it would be disadvantaged in a severe sense. Someone who went about producing any and every sentence without concern for anything else might be quickly institutionalized. We have then to account for the fact that a normal child acquires a knowledge both of proper sentences and of their appropriate use. He or she develops abilities to judge when to speak, when not, and what to talk about with whom, in what way, and when and where.

It is especially important not to confuse an account of such abilities with an account of performance. The broad theory, like the narrow theory, has both competence and performance aspects. Indeed, one of the chief dangers of leaving the field of linguistic theory to the narrow view is that it may encourage one to relegate all questions of use to the category of performance. As has been noted above, performance here amounts essentially to the exigencies of realization and interpretation in encoding and decoding. The abilities with which a broad theory of competence is concerned are in the first instance equally matters of underlying intuitive knowledge, of "mentalistic" competence, just as much as are the abilities with which grammer and semantics are concerned.[6] Moreover, although the notion of rules of use carries with it an indication of restraints, such rules are not to be taken just as limitations on an otherwise infinite capacity. First of all, such rules are not a late grafting. Data from very early in life, the first years of acquisition of grammar, show children to develop rules for the use of different forms in different situations (Susan Ervin-Tripp, personal communication). Competency for use is part of the same developmental matrix as competency for grammar.

Second, like competency for grammar, competency for use has a dimension of productivity. Within the developmental matrix in which children acquire the knowledge in principle of the set of sentences of a language they also acquire the knowledge in principle of a set of ways in which sentences are used; and they internalize attitudes toward a language and it uses, and indeed, toward language itself (including, e.g., attentiveness to it) or its place in a pattern of mental abilities.[7]

The words "in principle" in the last sentence should no doubt have been in quotes. No child has knowledge of all sentences, no more than he or she has knowledge of all applications of rules of use. The matrix formed in childhood continues to develop and change throughout life in both respects. Either or both may indeed be supplanted. Competency in either respect is not a matter of childhood alone, but of the succeeding stages of life as well.[8] Perhaps here one should contrast a "long" and a "short" range view of competency, the short range view being interested primarily in innate capacities as unfolded during the first years of life, and the long range view being necessarily concerned with the continuing socialization and shifting competence of lives through adulthood. In any case, here is one important respect in which a theory of competency must go beyond a narrow one, if it is to be of value to work with disadvantaged children. For when one is dealing with recurrently found differences, social in part or whole, with intent to change, one is presupposing the very possibility that competency that has "unfolded" in the "natural" way can be altered, perhaps drastically so, by environmental factors. One is assuming from the outset a confrontation of different systems of competency within the same community, and focusing on the way in which one affects or can be made to affect the other. In short, one's theoretical perspective can be limited neither to young children of pre-school age nor to homogeneous communities. One encounters linguistic phenomena that pertain not only to the structures of languages, but also to what has come to be called interference between them: problems of perception, understanding, and acquisition of habits due to the perception of the manifestations of one system in terms of the structures of another.

Since the interference one confronts involves language features and features of use together, it would be well to adopt the phrase introduced by Alfred Hayes into the Yeshiva conference last October, and to speak of sociolinguistic interference.

When a child from one developmental matrix enters a situation in which the communicative expectations are defined in terms of another, misperception and misanalysis may occur at every level. As is well known, words may be misunderstood because of differences in phonological systems; sentences may be misunderstood because of differences in grammatical systems; intents, too, and innate abilities, may be misevaluated because of differences of systems for the use of language and for the import of its use (as against other modalities).

With regard to education, I put the matter some years ago in these words:[9] "...new speech habits and verbal training must be introduced, necessarily by particular sources to particular receivers, using a particular code with messages of particular forms via particular channels, about particular topics and in particular settings--and all this from and to people for whom there already exist definite patternings of linguistic routines, of personality expression via speech, of uses of speech in social situations, of attitudes and conceptions toward speech. It seems reasonable that success in such an educational venture will be enhanced by an understanding of this existing structure, because the innovators' efforts will be perceived and judged in terms of it, and innovations which mesh with it will have greater success than those which cross its grain".

The notion of sociolinguistic interference is of the greatest importance for the relationship between theory and practice. First of all, notice that a theory of sociolinguistic interference must begin with heterogeneous situations, whose dimensions are social as well as linguistic, [situations of a sort from which the narrow theory seems in principle to cut itself off. (The fruits of such theory in the understanding of language systems can of course be *utilized* in dealing with sociolinguistic interference.)]

Second, notice that the notion of sociolinguistic interference helps one see how to draw on a variety of researches for practical purposes, researches that might otherwise be overlooked or set aside. (In saying "set aside", I have in mind the issues raised by treating the practical problems of education as problems in "second-language learning.") Now, one main virtue of the notion of sociolinguistic interference is that it fits into a conception of an *integrated theory of sociolinguistic description*. And such a theory of description does not begin with the notion of a language, or of counting numbers of languages, but with notions which have to do with codes and numbers of codes. In particular, such a theory of description recognizes that the historically derived status of codes as separate languages, related dialects, alternate styles, or whatever, is entirely secondary from the standpoint of their use in actual human relationships. From the functional standpoint that a sociolinguistic description must take, quite different means can be employed in equivalent ways for equivalent ends. A striking example from another area, that of modes of address, is that the function served by shift of second person pronoun in French, tu : vous, may be served by shift of entire language in some situations in Paraguay (Guarani : Spanish). In short, we have to break with the entire *a* language : *a* culture tradition of thought, a fixation that has dominated linguistic thought for generations and indeed centuries. In order to deal with the practical problems faced among disadvantaged children, theory must begin with the conception of the speech habits of a population. Within those speech habits, it may find one language, three languages; dialects widely divergent or divergent by a hair; styles almost mutually incomprehensible or barely detectible as different to the outsider; but these objective differences in terms of linguistic structure are secondary and do not tell the story. What must be known is the attitude toward the differences, the functional slot assigned them, the use made of the varieties so distinguished. Only on the basis of such a functional description can comparable cases be established and valid theory developed.

Now with regard to sociolinguistic interference among school children, much relevant information and theoretical insight can come from the sorts of cases variously labelled "bilingualism", "linguistic acculturation," "dialectology", "creolization," whatever. The value of an integrated theory of sociolinguistic description to the practical work would be that (1) it would attempt to place studies, diversely labelled, within a common analytical framework; and (2), by placing such information within a common framework, where one can talk about relations among codes, and types of code-switching, and types of interference as between codes, one can make use of the theory while perhaps avoiding connotations that attach to such labels as "second-language learning." (I say "perhaps" because of course it is very difficult to avoid unpleasant connotations for any terms used to designate situations that are themselves intrinsically sensitive and objectionable).

William Stewart's suggestion that some code relationships in the United States might be better understood if seen as part of a continuum of cases ranging to the Caribbean and Africa, for example, seems to me from a theoretical standpoint very promising.[10] It is not that most code relationships in the United States are to be taken as involving different languages, but that they do involve relationships among different codes, and that the full series illuminates the part. Stewart has seen through the different labels of dialect, creole, pidgin, language, bilingualism, to a common sociolinguistic dimension. Getting through different labels to the underlying sociolinguistic dimensions is a task in which theory and practice meet.

Let me now single out three interrelated concepts, important to a theory of sociolinguistic description, which have the same property of enabling us to cut across diverse cases and modes of reporting, and to get to basic relationships. One such concept is that of _verbal repertoire_, which John Gumperz has done much to develop.[11] The heterogeneity of speech communities, and the priority of social relationships, is assumed, and the question to be investigated is that of the set of varieties, codes, or subcodes, commanded by an individual, together with the types of switching that occur among them.

The second concept is that of _domains of language behavior_, which Joshua Fishman has dealt with insightfully in his impressive work on _Language Loyalty in the United States_.[12] Again, the complexity and patterning of use is assumed, and the focus is upon "the most parsimonious and fruitful designation of the occasions on which one language (variant, dialect, style, etc.) is habitually employed rather than (or in addition to) another."

The third concept is that of linguistic routines, sequential organizations beyond the sentence, either as activities of one person, or as the interaction of two or more. Literary genres provide obvious examples; the organization of other kinds of texts, and of conversation, is getting fresh attention by sociologists, such as Harvey Sacks, and sociologically oriented linguists, such as William Labov. One special importance of linguistic routines is that they may have the property that the late English philosopher Austin dubbed performative. That is, the saying does not simply stand for, refer to, some other thing; it itself is the thing in question. To say "I solemnly vow" is to solemnly vow; it does not name something else that is the act of vowing solemnly. Indeed, in the circumstances no other way to vow solemnly is provided other than to do so by saying that one does so. From this standpoint, then, disability and ability with regard to language involve questions that are not about the relation between language and something else that language might stand for or influence; sometimes such questions are about things that are done linguistically or not at all.

These three concepts do not exhaust those that are relevant to the sort of theory that is needed, and a number of scholars are developing related conceptual approaches, such as Bernstein, who has been mentioned, Harvey Sarles, who will speak later in the conference, and others. But the three concepts do point up major dimensions: the capacities of persons, the appropriateness of situations, and the organization of verbal means for socially defined purposes.

In the context of interference, let me take up another aspect of communication relevant to work with disadvantaged children. I have so far not justified the scope implied by the word "communicative" in my title, and in fact I shall continue to focus on language, since it is the center of our interest here. But let me introduce one principle with regard to interference that does call for the larger perspective of communication in general.

Phenomena of intonation, tone of voice, expressive phonetic features, and other parts of paralinguistics; phenomena of body style, gesture, and other parts of kinesics; all that Edward Hall designates as the "silent language" and the "hidden dimension"; these things need only be mentioned to be recognized. Yet it is remarkable how easy it is for us to forget them. In Dr. Cazden's review article, she makes an important critical point, namely, that a common finding may easily be given two quite different interpretations. The example cited may be evidence of the point I wish to make now. Bernstein has interpreted a greater use of "I think" among higher-status subjects in terms of egocentricity-sociocentricity contrasting with "ain't it", whereas Loban has taken a like result as evidence of cognitive flexibility (grouping it with "I'm not exactly sure"). The question arises: did Bernstein's English subjects say "I think" (egocentric) and Loban's

California school children "I think" (cognitive flexibility)? Clearly the import of data can not be assessed apart from the co-occurring set of intonational and expressive signals.

The question of communicative interference poses itself here in two ways. There is first the problem of interference between differing sets of expressive signals. Of this there are many examples in education and the transmission of information, e.g., Steven Polgar reported some years ago that Mesquaki Fox children near Tama, Iowa, interpreted the normal loudness of voice and directness of teachers as "mean"-ness and as getting mad.[13]

Second, there is the problem of interference with regard to relations between co-occurring codes within a single message. The principle of concern here can be put as an instruction: "Find out where the information is." A child is making use of a set of modalities, as he or she communicates, and interprets communication, and only one of them is discursive language. One of the essential features of Bernstein's model for restricted and elaborated types of codes is that the grammatical and lexical restrictiveness of the first type is accompanied by intensified perceptual activity with regard to other cues of subjective intent, such as the paralinguistic. (I may mention that I have found Bernstein's model very useful cross-culturally). In such a case the two parties to a communicative exchange may be putting their main information in different places, and likewise looking for that of the other in different places. The situation is further complicated by what the late Dutch linguist de Groot called "the law of the two strata", namely, that when the discursive and the expressive import of a message conflict, the latter signals the real intent. Quite possibly some teachers are not reading their students at all, and some children are reading their teachers all too well. In any case, a theory of competence that is to be of much help in assessing an array of signals and a battery of functions, such that what is signalled lexically in one case may be signalled with expressive intonation in another, and so on. The theory of competence can not be limited to the referential use of language.

Here indeed is the point at which the sort of theory of competence one needs must depart most decisively from the orientation of the sort of theory first discussed. When one takes into account the full set of functions served in speech in relation to the means diversely organized to serve them, one's starting point and orientation shift. A linguistic theory in the narrow sense, in so far as it deals with use, looks out from language; structure precedes, functions of use follow. A theory in the broad sense looks in at language in the contexts of its use; functions guide, structures follow.

Such a broad theory of competence is essentially sociolinguistic. As such, it makes three assumptions:

(1) each social relationship entails the selection and/or creation of communicative means considered specific and appropriate to it by its participants;

(2) the organization of communicative means in terms of social relationships confers a structure that is not disclosed in the analysis of the means separately;

(3) the communicative means available in a relationship condition its nature and outcome.

These three assumptions are rather simple and obvious, but to take them seriously is to define an area of linguistic investigation almost wholly unsystematized and theoretically little understood. To take the first assumption: a social relationship gives rise to a use of communicative means that distinguishes it. Now it is probably a sociolinguistic universal that the speech of men and women can be distinguished in every society. Yet articles on men's and women's speech are few; they are also very revealing. They deal with men's and women's speech when markers of the distinction intrude themselves into the ordinary analysis of the language. For the vast majority of societies where the markers have not so intruded, we are largely ignorant.

The fact is evidence of the second assumption: the way communicative means are organized in terms of a social relationship is unlikely to appear unless one begins with the social relationship, then looks for the means.

The third assumption is perhaps the simplest, the most obvious, and for some reason, the most resisted by some linguists. Put colloquially, it says with reference to language that what people have to work with affects what they can do. In it lies the heart of the element of truth in what is often called the Whorfian hypothesis. Partly the question is one of performance, as brought out by Cazden:

"When we shift...to the difference between the speech of a middle-class child and a lower-class child, however, we are looking not at the total available in the language as a system of symbols, but at what is actually used by particular persons at the moment of constructing an utterance." In important part the question is also one of competence, a competence which is in part an individual matter (cf. Sapir, "every individual's language is a distinct psychological entity in itself")[14], and in part a matter of social group. Each child in a classroom has a competence definable in terms of what is normally and habitually available to it for utterance and comprehension, a competence partially unique to it, largely shared and predictable in terms of its social origins and experience, and never identical with that of a dictionary, a grammar, or an ideally fluent speaker-listener.

With regard to disadvantaged children, the goal of an integrated theory of sociolinguistic description would be to guide accounts of the range of settings, function, and means, and their interrelationships, acquired by the children. Of these the school setting would be one, but not the only one; and major purpose would be to place the school setting in the context of other settings, so as to delineate the true communicative abilities of the children and to show the extent to which the performance in school settings was not a direct disclosure of their abilities, but a product of interference between the system that they bring and the system that confronts them; or a setting simply largely irrelevant to the direction their abilities and competence otherwise took. In part the problem is one of conflict of values and of perceived interests. Indeed, since the beginnings of stratified society and the use of writing, it has been characteristic of much of mankind that a desired or required linguistic competence has stood over against men, as an alien thing, imposed by a power not within their control. Even in the simplest case, of course, sociolinguistic competence is achieved along specific lines, not merely released. In the complex circumstances of our own society it is hard to see how children can be expected to master a second system, complementing or replacing their own, if the process is not perceived as intrinsically relevant, or enjoyable (preferably both).

Much more needs to be said and done with regard to the conceptual content of sociolinguistic description, regarding interference, competence, etc. In other writings I have outlined schemes for "the ethnography of speaking", or "ethnography of communication", together with some notes and queries about children's acquisition of language; I shall not go further into that here.[16] Rather let me sketch what might illustrate a practical framework for the use of a sociolinguistic description.

As Dr. Gordon reminded us at the earlier conference, it is hardly our task to say what the goals of the disadvantaged should be. If one prime consideration is to be chosen, probably it is jobs. From this standpoint, a rough scale can be defined in terms of the concepts of repertoire, domain, and routine. For each one asks how many and what kind, moving from the minimal to the maximal requirements for use of a more-or-less standard set of speech habits. For purposes of the scale, the single concept of fluent speaker is replaced by a rough division into *fixed*, *flexible*, and *fluent* (or *facile*) use.

	Repertoires	*Domains*	*Routines*
Fixed			
Flexible			
Facile			

The minimal competence (lowest rung of fixed) could be characterized as use of a single routine in a single domain without need to switch within one's repertoire. Additional considerations might be that the channel be writing, thus permitting revision and correction somewhat at leisure, and that the demands on the one part of the repertoire be of the transactional or restricted code sort. Jobs of this sort are probably today mostly taken care of by form letters, or, in the vocal sphere, by recordings, to be sure. Perhaps the need only to receive not to send, might be added to define the minimal rung.

The maximal competence (facile) could be characterized as use of multiple routines in many domains with facility at switching between parts of one's verbal repertoire, both sending and receiving.

Medial competence (flexible) could be defined in terms of the empirical situation, if some intermediate set of needs and abilities with regard to routines, domains, and repertoires can usefully be recognized.[17]

Some such scale could be used to conceptualize and analyze the requirements of situations, such as types of jobs; the capacities of persons; the aims and levels of a program of training.

What sorts of interference may occur, what sorts of learning and change may be required, cannot of course be postulated in advance. Sometimes the question will be one simply of dialect markers, of the social rather than the referential or expressive information called for in the situation. (My own quite unrealistic preference would be to leave dialect alone, insofar as markers are all that is involved). Sometimes the question will be one of added skills in the use of syntax or narration; and so on.

(Ultimately I should hope that concern for language use might get to the aesthetic and clarifying and truth-telling roles it plays in our lives, and that we might someday have a conference on the ways in which middle-class and verbally fluent individuals are disadvantaged. A critique of the use of language among the disadvantaged would indeed not be hard to mount, and there are even some who argue that a withdrawal from the ordinary uses of language altogether is under way in rebellion. But no government is about to spend much money to get the government to use language in a more satisfying, beautiful, clarifying, or truthful way.)

Let me conclude by summarizing the way in which concern with language use among disadvantaged children fits into the present stage of linguistic theory.

First, it is of course not mandatory that the term "linguistic theory" be used in one particular way. If one wishes to reserve "linguistic theory" for the narrower sort of competence, then "sociolinguistic theory" will do for the broader sort of competence. What is essential is that concepts of the nature of language and its use not be preempted in the name of "linguistic theory" by a narrow view. The understanding of language use involves attention not only to participants, settings, and other extra-linguistic factors, but also attention to purely linguistic phenomena, and the discovery and statement of new features, organization, and relationships in the data of language itself, when viewed from the more general perspective of social relationships. What is essential is that conceptions of speakers, listeners, and competency, take into account as quite normal in the world the situations of diversity of codes; see the child as acquiring and indeed, achieving, narrowly linguistic and broadly sociolinguistic competence together.

In this regard a sociolinguistic theory is not a departure from past linguistic insight. The narrow theory earlier discussed has known how to *reculer pour mieux sauter*. It has found in von Humboldt, and more recently in Jespersen and Sapir, instances of fresh insight into the structuring of language which it wishes to renew and to capitalize. Sociolinguistic theory is in an analogous position. In von Humboldt it finds not only a generative conception of rules, but also a concern with the individual worlds created in and through language; a concern not only with universals, but concern also with the particulars in which they are embodied; a concern with the infinite capacity of man that implies also the determinate form such capacity requires for realization in each person; an understanding of human nature, the human essence, as not so much a state of being, as in each case a unique existential achievement.[18]

In Jespersen it finds a grammarian who devoted himself to universals, productivity, and to understanding mankind, nation and individual from a linguistic point of view. In Sapir it finds a pioneer of structuralism, the autonomy of linguistic form, and proper insight into phonology, who also urged that:[19]

> "It is peculiarly important that linguists, who are often accused, and accused justly, of failure to look beyond the pretty patterns of their subject matter, should become aware of what their science may mean for the interpretation of human conduct in general. Whether they like it or not, they must become increasingly concerned with the many anthropological, sociological, and psychological problems which invade the field of language."

There is under way now, I think, a shift in emphasis in linguistics, one that is partially completed, and which the work with disadvantaged children may help to complete. The emphases can be shown in terms of two dimensions: one distinguishes language structure and function, and one distinguishes the study of a single language or community from comparative perspective.

	Single case	Comparative
Structure		
Function		

The earlier set of emphases can be said to have been (from left to right, and first top, then bottom), with regard to the structure of a single language, find the invariance; with regard to structures comparatively, find diversity; with regard to functional aspects in a single case, assume diversity, and the variation of parole (and assign anything that interferes with the invariance and system of structure to this category); with regard to functions comparatively, assume invariance (the functions of language are universal; all languages are functionally equal).

The emerging set of emphases can be said to be, with regard to the structure of a single language, find the variation with regard to structures comparatively, find the invariance. The relationships of emphasis in other words are reversed. Both these emphases are well established now in the new interest in social dialect, linguistic varieties, styles and levels, on the one hand, and the different approaches to universals of language on the other. The rest of the new set of emphases, reversing the former set, is only coming to be realized: with regard to functional aspects of a single case, find the invariance (the sociolinguistic system); with regard to functions comparatively, find the diversity (take the functions of language, or of a language, as problematic for any given group).

It is precisely with regard to these last two sectors that the problems of the study of disadvantaged children and the needs of theory converge. The understanding of sociolinguistic systems as a basis for handling interference between them, and the non-identity of the functioning of language in different social groups are problems common to them both. Perhaps this common interest can help to end the division between linguistic theory and the concrete, existential human world, the world of actual human relationships, that has dogged the study of language for so long.

FOOTNOTES

[1] Noam Chomsky, Aspects of the theory of syntax (Cambridge: M.I.T. Press, 1965), p.3.

[2] A continuity with more recent American structural theory is seen in the concern with what linguistics is taken not to include, as reflected in attitudes toward words that tag the external. In the heyday of Bloomfieldian linguistics "meaning" and "mentalism" were dirty words; today, for some, "context" is.

[3] Leonard Bloomfield, "Literate and illiterate speech", American Speech 2: 432-439 (1927); reprinted in D. Hymes (ed.), Language in culture and society (New York: Harper and Row, 1964), p. 395.

[4] Courtney B. Cazden, "Some views on subcultural differences in child language," *Merrill-Palmer Quarterly*.

[5] William Labov, "Stages in the acquisition of standard English," in Roger W. Shuy (ed.), *Social dialects and language learning* (Champaign: National Council of Teachers of English), pp. 77-103; see p. 102, n. 12.

[6] Cf. Labov on the priority of subjective evaluation over performance in social dialect and process of change, pp. 84-85.

[7] Cazden, pp. op.cit.

[8] Labov, pp. 77, 91-92.

[9] Dell H. Hymes, "Functions of speech: an evloutionary approach," in Frederick C. Gruber (ed.), *Anthropology and education* (Philadelphia: University of Pennsylvania Press, 1961), pp. 55-83; see pp. 65-66.

[10] William A. Stewart, "Urban Negro Speech: sociolinguistic factors affecting English teaching," in Shuy (ed.), pp. 10-18; see p. 11, n.2.

[11] John J. Gumperz, "Linguistic and social interaction in two communities," in John J. Gumperz and Dell Hymes (eds.), *The ethnography of communication* (American Anthropologist, vol. 66, no. 6, part 2 (December 1964), pp. 137-153.

[12] Joshua A. Fishman, *Language Loyalty in the United States* (The Hague: Mouton and Co., 1966), pp. 424-438; see p. 428.

[13] Steven Polgar,
American Anthropologist 62:

[14] Edward Sapir, "Language", *Encyclopedia of Social Sciences* 9: 155-169 (New York: Macmillan, 1933); cited from D. G. Mandelbaum (ed.) Selected writings of Edward Sapir (Berkeley and Los Angeles; University of California Press, 1949), p. 23.

[15] Labov, pp. 96-99, discusses opposing value systems in the New York speech community.

[16] Hymes, "The ethnography of speaking," in *Anthropology and Human Behavior*, Washington, D.C.: Anthropological Society of Washington, 1962, pp. 13-53; "Introduction: toward ethnographies of communication," in *The Ethnography of Communication* (Washington, D.C.: American Anthropological Association, 1964), pp. 1-34; "Linguistic aspects of cross-cultural personality study," in *Studying Personality Cross-Culturally*, ed. by Bert Kaplan (Evanston: Row, Peterson, 1961), pp. 313-359.

[17] Labov's sixth level of the acquisition of spoken English, *full range*, would be maximal with respect to switching within repertoire and among domains, but perhaps admitting degrees with respect to routines. His fifth level, *consistent standard*, seems to define a medial area of some flexibility short of complete facility.

[18] Ernst Cassirer, *The logic of the humanities*. Translated by Clarence Smith Howe. (New Haven: Yale University Press, 1961), pp. 19-26, discussing Herder, Goethe, W. von Humboldt.

[19] Edward Sapir, "The status of linguistics as a silence," *Language* 5: 207-214 (1929) pp. 214 (the concluding remark).

Cazden's Commentary on Hymes Paper

Since I am not a linguist I am not going to discuss questions of linguistics. Fortunately the group here includes linguists of various theoretical points of view, Wayne O'Neill and Bill Stewart to name two, and so I know that issues of linguistics itself will get picked up and talked about by people more competent than I. As a person who has been concerned with disadvantaged children, I completely agree with Dell's feeling that linguists have not been particularly helpful so far to people like myself in defining the relationship between the language of disadvantaged kids and the problems-- cognitive, communicative, and so forth-- that they have in school. Linguistic descriptions of, let's say, dialect differences, do seem to be largely irrelevant to the problems that kids have in school. Consequently, I was very struck by Dell's use of the term "a doctrine of poignancy." I come from Cambridge where the transformational view of linguistics is very prominent, and it has been my feeling in trying to bring together the insights that the transformational point of view has given us with the hard facts of disadvantaged kids and their problems, that this does become a doctrine of poignancy. We have here an enormous human potential which for reasons that we don't yet understand is not realized in ways that enable the child to cope with what faces him in school. First, what is the nature of the difficulties; second, how do they come about; and third, what do you do about them, are still with us as major problems-- "what happens in the fallen world", as Dell said.

And I was also very struck by the general thrust of his paper, that the attempt itself to describe more completely the language abilities of these children, and the problems they face in school, may have a very salutary effect on linguistic theory as a whole-- this is something that I really hadn't thought about. Let me just here confess a misconception which I had. In the first place there's a difference between competence and performance, but I must admit that I had all along thought that if you put grammar over on the competence side, then use was over in the performance category. In other words, if the child's knowledge of what is a grammatical sentence and his ability to produce and understand novel grammatical sentences is one of the things we mean when we talk of linguistic competence, then my misunderstanding was that questions of language use fell on the performance side. However, I understand what Dell was saying this morning to mean that we have to consider questions of usage as part of linguistic competence along with matters of phonology and grammar. That as a part of his linguistic competence a child learns "what is appropriate to say when and to whom and about what."

Now we have talked for a long time about the kinds of interference that may be present between the grammar that the child has learned, the particular rules of his dialect, and the grammar-- the rules of the language-- in his school books, or that his teacher uses, or that the children in his peer group use. We have assumed that there may be a kind of linguistic interference here that makes it difficult for him to learn to read the books or learn to understand what his teacher says. We've talked for a long time about this kind of interference on a phonological level or on a syntactical level; and controversies rage as to how significant that interference is, and what it means for the preparation of beginning reading materials. But what Dell added this morning, at least added to me, was that we also have to talk about what he calls socio-linguistic interference operating at the competence level-- that what the child has learned in his natural speech community, his home, his peer group, his neighborhood, about what is appropriate to say to whom and under what conditions, is as much a part of his competence as his grammar, and that there may be interference in this area between what he brings and what the school demands as well as in the areas of grammar and phonology and other things that I think we are more used to thinking of in this way. Now, how you go about finding out what it is he brings, and what it is the school demands and the degree of match or mismatch between them, this it seems to me is the critical question and I'm still left wondering how to do it. And I hope that some of the discussion during this conference will get at the "how to do it", and will suggest the kinds of studies we should be doing to get at this question.

Discussion following Cazden's commentary

Stewart: I just wanted to clarify something. I don't think you can separate grammar or structure, particularly linguistic structure and use, because what you're dealing with is competence and performance in linguistic structure and in the use of that linguistic structure, and many questions of use involve changes in linguistic structure. Now if you separate out grammar and use then it appears that dialect differences are less important for classroom problems.

However, many of the problems in the school involve problems of the necessity to shift various kinds of phonological, syntactic and lexical behavior with respect to the language domain and if you look at language use as the use of alternative linguistic structures, then linguistic differences within a single dialect again become crucial. So I don't think it's helpful to separate grammar and to leave use as some sort of separate level.

Gumperz: Like Bill Stewart I want to emphasize the fact that in shifting from the study of linguistic structure to the study of socio-linguistic systems we should not disregard some of the important advances that have been made in syntax and in linguistic structure. In other words, if the use of code resources within the repertoire is determined by socio-linguistic factors, it seems to me the important question is what are these socio-linguistic factors. And I think we could specify some of the types of research that are needed. If a repertoire is a set of styles, dialects, languages, there must be a set of rules which determine the use of these dialects, styles or languages, and this requires a social theory as well as a linguistic theory. Now one such type of social theory Dell has alluded to. He says that "social relationships determine the selection of communicative resources." What are these social relationships? Ward Goodenough has recently published an article "Role and Status Re-examined" in which he develops an abstract language of roles and statuses and the beginnings of a syntax of roles and statuses. He asks such questions as "if a physician and patient interact, what kinds of social identities are brought into play?" He says the relationship physician-patient is valid in the case of somebody going to a physician's office, but when this same physician interacts with his wife, the relationship physician-wife is not grammatical in our culture-- what we have is a husband-wife relationship. In other words, what we will have to develop is a language-- an abstract set of concepts of analysis of roles and statuses, and it seems to me that this can be handled by the same kinds of methods by which linguistic analysis is now handled and can be related to grammar in the same way that we relate morphology to syntax. We use concepts such as representation or realization. We say that grammatical rules are realized phonologically. Now it seems to me that the crucial thing in socio-linguistic investigation is to find out how social rules are realized linguistically.

I think the actual linguistic situation or the repertoire is crucial in this sense because the structure or the code variety within the repertoire determines how these social rules are realized. If, for example, the distinction between solidarity and distance in Paraguay is expressed by the shift from Guarani to Spanish, in France it is expressed by the shift from tu to vous. Furthermore, shifting can be of two kinds. It can be strictly predictable on the basis of social situation as is the case, for example, in military terminology where it is predictable when a private will say "Sir" and when he will use first name. There is a strict code of statuses which he has to learn. Now this is predictable shifting. Then there is a flexible kind of shifting that exists, let's say, at a party where we have a certain kind of leeway. So there is variation within the same culture as to whether there are strict selection rules or whether the individual is left with choices. In other words, there are differences in the degree of socio-linguistic choice, the degree of socio-linguistic flexibility left to an individual. One of the things that seems to me characteristic of educated speech is that it has a maximum of socio-linguistic flexibility: whereas I have the feeling that people who are less a part of the educated world are frequently more rigid and inflexible. In other words possibly what we should be teaching is flexibility-- at least it's something we have to find out. What we need then is not less linguistic work, not less work on syntax-- but ethnographic work as well. We have to develop a theory of social relationships which is much more explicit than whatever theory we have right now.

Hymes: I'm glad John brought up this point. In stressing the socio-linguistic aspect I certainly was not saying you don't need to worry about linguistics. Bringing in the social approach just makes life more difficult, not easier, and any advance in linguistics may be crucial for understanding what's going on. One other point. We always tend to think of the lower class person as the handicapped one and of course it's just not true. There are a lot of people who speak a beautiful form of English from the point of view of dialect markers, but who have severe problems of rigidity in their use of language-- these functional categories cut across the overt markers of social group.

Sapon: I have a brief question, Dell, Can you give a thumbnail definition of competence as contrasted with a thumbnail definition of performance?

Hymes: Basically, in this context, the idea is that performance is what is overtly observable, manifested in outward behavior. It includes such things as what's said on some actual occasion or series of occasions. As I understand the import, the thrust of the distinction is to say that one cannot restrict oneself to the performance, that beyond and behind the finite observable data that the speaker may manifest, there is an underlying ability which is more general and more powerful, that goes beyond any particular set of things that may actually get said. It is a capacity that goes beyond the sort of existential actualities of finite speech of circumstances and includes a potential, part of which may never be realized. It's an anti-behaviorist argument, of course, that competence is mentalistic as opposed to behavior. . .

Sapon: That's the question I have to ask-- doesn't it make you feel uncomfortable as a scientist? I gather from what you say that performance relates to what people do and competence relates to what they can do, and the way we determine what they can do is by what they do. I have a very strong feeling of urgency for the purpose of this assembly and my concern here has to do with the empirical testing of our intervention procedures. It may make for a very very happy world to say that an organism performs in a certain way only because he must have internal in his nervous system certain patterns-- that is, the only way I can satisfactorily explain his behavior is to impute this back into the organism. But putting it back into the organism takes it out of the realm of empirical tests, out of our experimental world. My concern here is with the issue of where we intervene. Much has been said about intervention in the direction of competence so that we are aiming at something that has been hypothesized, logically arrived at, and put back inside the nervous system of the organism where it is totally beyond reach.

Hymes: There might be many uses of the notion of competence which rightly would deserve a behaviorist critique, the ground being that they are imputing competence on too little evidence. That would be a question of the particular case, not of the notion. But we do have good evidence in fact to impute to an individual some such ability on the basis of what we've actually observed, and though one can be worried about doing this too readily, I don't feel able to do without the notion by just this sort of ad hominem argument. When Dr. Bailey invited me I guess she had some idea there was some sort of underlying competence, something beyond actual performance, since she's never heard me talk so far as I know.

Sapon: Perhaps you are introducing a new definition of competence which might make some people feel less uncomfortable-- i.e. that competence has to do with the ability to predict the behavior of individuals with whom we are concerned. Clearly Dr. Bailey had some underlying notion of competence, but I suggest that what this meant was that she was able to predict some things about the kind of performance you would display, and such predictability is measurable.

Hymes: I can go along with that since from the point of view of the kind of anthropological thought with which I associate myself, the notion of predictability is the criterion for imputing an underlying regularity or potential.

Sarles: I want to switch the grounds of this argument. If Professor Hymes had walked in known only by reputation and not by sight, unshaven and wearing old clothes, I would guess that the credibility of his performance would be very different. Competence in fact is not only in the performance itself, but in the mind of the audience and the person that's talking.

Shulman: One of the distinctions that is apparently still working here is the behavioristic-mentalistic one. Psychologists have begun to stop using it because about a generation ago they found that they couldn't really account for differences in performance now which did not accurately predict differences in performance later without beginning to talk about something called mediators. The Kendlers have generated a paradigm to study concept levels by using a transfer design in which you identify apparent similarity of performance now, but predict difference in performance later on the basis that the subject is using different mediators to produce the same performance. I think linguists may need more transfer designs-- they need to think of their study as change rather than in terms of the specific thing that's going on right now.

Gumperz: I want to point out one important consequence of putting the study of use in the theory of competence. What this really means is that we have to collect two kinds of data. What has happened in linguistics with the emphasis on competence, is that we are beginning to develop new kinds of data collecting. We're not simply listening to what people say-- we're not simply taking down text, but we're asking people to perform certain tasks, we're using problem solving techniques in addition to observational techniques. I think the implication is clear that if we put the study of use into this category of competence, we will need not only to observe what people do, but to have other techniques-- batteries of tests, problem solving, possibly the study of social norms and attitudes-- for getting at competence. We can't simply continue to observe.

Stewart: I want a terminological clarification. Not to change the established terminology, but just to make it easier for people to understand this dichotomy between competence and performance and what is called grammar and use. If we substitute language structure for what's called grammar, you can think of the total inventory of language structures available as competence and the generation of specific utterances as performance. Then you can think of the total inventory of knowledge about how you use these linguistic structures (and notice that you can't separate language use from language form) that are available as competence and the application of this knowledge of use to the particular social situation as performance. If you think of it that way you get a better idea of how language structure and use on the competence side relate to actual application or generation on the performance side.

One thing I'd like to point out is that we've got to be very careful in the assumptions we make about what language competence is for a given social group, a given community, and especially a given individual, on the basis of observations of the performance type in other individuals and in other social groups, no matter how much these may appear to be related. There's been a lot in the literature lately which makes the assumption that children in large metropolitan areas because they come into contact with standard speakers have within their competence repertoire, knowledge of the standard linguistic structure, and that really the matter of non-standard speech is a question of performance constraints, or conventions of use. I think this is highly questionable as a generalization. There are many ways that people can interact and understand other people without having competence in the standard structure. He may understand phonological distinctions which he cannot use because he is using other criteria for understanding, eg. contextual criteria. The fact that a non-standard speaker understands the standard speaker does not mean necessarily that he's got built in at some area of competence a recognition of these phonological distinctions. The one danger in this business of going from observations of performance to theoretical assumptions about competence is that we've got to be very careful not to transfer our assumptions about competence to other individuals.

Sapon: That was the point I was making when I said that two people who had by one set of analyses the same performance, did not indeed have the same performance by a more sophisticated set of analyses. If you'll forgive the behavioral terminology for a moment, you're saying that two different pieces of behavior identified as sames that occur under two different sets of control stimuli are not indeed sames. You may very will need two different sets of analytical descriptions to talk about the person's receptive language and to talk about his productive language. We get into serious involvement here by talking about language as if it were a unitary thing.

How To Learn a First Language

David McNeill

My title suggests that I have some worthwhile advice to give to two-year old children who are attempting to acquire their native tongue. In fact, however, the content of my talk moves in precisely the opposite direction. Rather than present advice to two-year-olds, I hope to discover what it is that allows them to acquire language as rapidly and as successfuly as they do. My concern, indeed, is with those aspects of language acquisition that never require advice, guidance, or assistance.

The argument will be that children are biologically endowed with a specific capacity to acquire language, and that this capacity, combined with the speech that children receive from their parents, automatically results in the acquisition of syntax. In short, the argument claims that children cannot avoid acquiring a language.

I will not attempt to develop the implication of this view for the linguistic development of disadvantaged children, except to state at the outset that the notion of a capacity for language is taken seriously. Which is to say that disadvantaged children and advantaged children alike are presumed to be endowed with the same capacity for language, and that both presumably acquire language in the same way. It seems important to characterize this capacity in discussing the linguistic development of underprivileged youngsters, and this for two reasons. One is the possibility that remedies can be devised to exploit the capacity for language of underprivileged children. The other is the possibility that the precise nature of the disadvantage suffered by underprivileged children can be better specified by describing what they are not deficient in, namely, their inborn capacities.

The view argued here is certainly not new. It was taken for granted in the 19th century (Hale, 1886), and it has long been part of the rationalist theory of mind (see Chomsky, 1966). However, the hypothesis that there is an "instinct for language-making", as Hale called it, is not particularly interesting. It merely restates the observation that man is alone among all creatures in possessing language, and it omits mention of what the specific characteristics of this capacity might be. On this point, however, some fairly definite, though unfortunately limited, suggestions can be made.

Let us consider for the moment, not children, but an abstract Language Axquisition Device (Chomsky, 1965). I shall call it LAD (or if you prefer, a Language Acquisition System--LAS--the feminine form).

LAD receives a *corpus* of speech--some set of utterances. The corpus is large, but *it is* not unlimited in size. Let us say that it contains the total number of utterances ordinarily overhead by a two-year old child. On receipt of this corpus LAD formulates a *grammatical system* of some kind. The grammatical system, in turn, can be regarded *as LAD*'s theory about the regularities that appear in the corpus of speech. It is, in fact, LAD's grammatical competence in the narrow sense--his grammatical knowledge (ef., Hymes contribution to this conference).

LAD constructs a theory by passing the evidence contained in the corpus of speech through some kind of *internal structure*. The sequence of events therefore is as follows:

CORPUS → $\boxed{\text{LAD}}$ → GRAMMATICAL SYSTEM (Competence)

If we understood LAD's internal structure, we would then understand how LAD constructs a grammar. Our problem is much like those exercises given to engineering students where they must infer the internal wiring of a black box from its various input-output relations. In our case, LAD is the black box; like an engineering student, we have to develop a theory about its internal structure.

One clue to LAD's internal structure arises from the fact that it must be able to acquire any language. LAD should not find Bantu, say, easier than English, or Japanese, or Russian. Whatever makes up the internal structure of LAD must, therefore, be universally applicable. Which is to say that LAD may contain information relating to those linguistic features that are universal, but must contain *no* information relating to those features that are linguistically unique.

Those of you familiar with recent work in transformational grammar will recognize that LAD's internal structure can be described by the so-called *theory of grammar*. The theory of grammar deals with the general form *of human language*--with the features that appear in natural languages everywhere, regardless of physical or social setting. If LAD were endowed with knowledge of universal linguistic theory, it could then restrict its attention when developing a grammatical system to acquiring those features that are *not* universal. Thus we have one hypothesis about LAD: its internal *structure* can be described, in part, by the universal theory of grammar, and the outcome of LAD's activity is the grammar of some particular language.

Conceiving of LAD in this manner will help clarify the acquisition of language by real children as well as by abstract ones. The two pose exactly the same problem. Like LAD, children are exposed to a corpus of speech, and like LAD, they develop grammatical competence on the basis of this corpus. Moreover, in the case of both LAD and children, some kind of internal structure converts the corpus of speech into grammatical competence.

Since the same corpus is input and the same grammatical system is output, LAD and children have the same internal structure.

To conclude otherwise would merely be perverse. In short, LAD's internal structure corresponds to children's capacity for language, and the theory of grammar, being a hypothesis about LAD, is also a hypothesis about children's innate capacities. Languages possess the universal properties contained in the theory of grammar just because all languages are acquired. The renewed formulation of grammar by each generation of children automatically imposes features on language that correspond to children's capacities. Such features therefore appear universally, and the theory of grammar is possible.

Although the theory of grammar is logically sufficient to describe children's capacity for language, the theory is far from being completely formulated. However, it is advanced far enough to reveal what some of the universal characteristics of language must be. Of these, I will take time to mention only one--the existence of the so-called basic grammatical relations.

The basic grammatical relations are the concepts of the *subject* and *predicate* of a sentence, the *main verb* and *object* of a verb phrase, and the *modifier* and *head* of a noun phrase. The grammatical relations can be very simply defined in linguistic theory (Chomsky, 1965), which is to say that they are held to be part of the general form of human language, and so, are held to be part of children's innate capacities. The definitions apply to the underlying structure of sentences, before any transformations have been applied, which means that they are *not* presented to the children in parental speech. The basic grammatical relations are defined in the underlying structure for the reason that they can be systematically applied there and nowhere else. To take one of the standard examples, the two sentences, *John is easy to please* and *John is eager to please*, both have the word *John* as the initial noun phrase. This is true of their surface, or manifest, structure. However, it is obvious that the word *John* plays different grammatical roles in the two sentences. It is the *object of the verb* in the first, and the *subject of the sentence* in the second, two facts obscured by the identity of surface structure. The underlying structures of these sentences, on the other hand, differ in just the right way for the definitions of subject and object to apply.

The point for language acquisition is that *there is no way for a child to infer the basic grammatical relations from presented parental speech*. Parental speech must mislead a child on this point. But since these definitions are part of the theory of grammer, they reflect an aspect of children's capacity for language. We should therefore expect to find them honored in children's earliest efforts to produce grammatical speech, even though there is no possibility that a child could infer these relations from the corpus his parents provide. Our problem now is to see if such evidence exists.

Let me first take up the case of a child acquiring English. The child is one of Roger Brown's subjects, a little boy whom he has been calling Adam. At the time Adam's speech was first recorded, at 27 months, his vocabulary appeared to be organized into three grammatical classes--verbs, nouns, and a so-called pivot class. The evidence for these classes was distributional. Words, which in English would be classified as verbs, had privileges of occurrence in Adam's speech different from words, which in English would be classified as nouns. The pivot class had a third privilege of occurrence, but it was grammatically heterogeneous from the point of view of adult English. It contained articles, demonstrative pronouns, possessive pronouns, adjectives, and such words as *other*, and *another*.

An adult listening to Adam's speech at this time would receive the strong impression that the basic grammatical relations were honored. Sentences like *change diaper*, *want milk*, and *truck hit* appear to have subjects, predicates, or objects. However, this impression could be entirely wrong, for it is at least possible that adults do not always understand what children intend to say, in which case it would be incorrect to impute the basic grammatical relations to their speech. Some other method of analysis must be used in order to avoid this logical circularity.

One approach is through the following arithmetic. With three grammatical classes, nouns, verbs, and pivots, there are $(3)^2 = 9$ different possible combinations of *two* words, and $(3)^3 = 27$ different possible combinations of *three* words. However, not all these 9 and 27 different combinations *are* direct manifestations of the basic grammatical relations; that is, not all are sequences of classes that result directly from the definitions of the basic grammatical relations contained in linguistic theory.

In fact, only four of the 9 possible two-word combinations meet this condition. The remaining 5 are "inadmissible". An example of an admissible combination is N + V (*Adam run*), which corresponds to the subject-predicate relation. An inadmissible combination is V + V (*come eat*). Among the three-word combinations, only 8 of the 27 possibilities are direct manifestations of one or another grammatical relation and the remaining 19 are inadmissible. An example of an admissible combination is V + N + N (*change Adam diaper*), whereas an inadmissible combination is V + V + N (*come eat pablum*). For details, see McNeill (1966).

The first three samples of Adam's speech contained examples of every admissible combination, but *no* examples of inadmissible combinations. All 400 sentences comprising Adam's corpus were of the admissible type. Thus, although *change Adam diaper* might have occurred, *come eat pablum* did not.

This outcome is not obvious, on either *a priori* or empirical grounds, and one might have expected matters to have been different. The surface structures of adult sentences present many examples of inadmissible combinations of Adam's grammatical classes. For example, the sentence type represented by *come and eat this pablum* is surely common in the speech directed to children. To judge from some experiments with artificial languages by Braine (1963), adults find it difficult to *avoid* learning patterns to which they are exposed, even if told that the patterns are *not* examples of what they are to acquire. Assuming that the same sensitivity holds true of young children, then some explanation must be offered for the fact that Adam did not say *come eat pablum* after hearing examples like *come and eat this pablum*. The explanation that suggests itself is that Adam was attempting to express the basic grammatical relations, but he did not yet have the transformations in English that allow them to be expressed in other than a direct way. A sentence like *come and eat this pablum* did not serve as a model because it does not directly manifest the basic grammatical relations. Instead, it is an example of a sentence in which the definitions of the basic grammatical relations are violated in the surface structure, although maintained in the deep structure.

To summarize, the basic grammatical relations are apparently observed in the earliest grammatical constructions of one child acquiring English. I want next to present evidence bearing on the basic grammatical relations taken from children acquiring a language other than English. If the basic grammatical relations reflect an aspect of children's capacity for language, then all children--regardless of their linguistic surroundings--should reveal the basic grammatical relations in their earliest speech.

This past year I have been collecting samples of speech from two children, both girls, who live in Tokyo, Japan. Each child is visited at home, twice a month, at which time everything spoken is tape recorded. The results to be described below are based on the corpus of one of these children, she was, at the times in question, 27 to 28 months old. In the interest of maintaining Brown and Bellugi's (1964) tradition for naming subjects in these studies, I call her *Izanami*, after the goddess in Japanese mythology who helped create the world.

In contrast to English, Japanese is a postpositional rather than a prepositional language. Among the Japanese postpositions are two, *wa* and *ga*, that mark, in the surface structure of sentences, the subject noun phrase. The presence of these postpositions is obligatory; they have the same distribution in the surface structure; however, they do not have the same significance--a matter to which I return later.

Wa and *ga* are both introduced into the surface structure of Japanese sentences by transformations (Kuroda, 1965). However, the two transformations do not operate on the same configuration in the deep structure of sentences. Only one, the *ga*-transformation, is related to the structure that defines the grammatical subject of a sentence. The *wa*-transformation has a different history altogether. Japanese thus presents a natural experiment: since *wa* and *ga* have the same superficial distribution but different underlying distributions, we can pit these two aspects of sentence structure against one another by determining if one postposition is acquired before the other, or if both postpositions are acquired at the same time. Because a transformation is a relation between the deep and surface structure of a sentence, acquisition of a transformation signifies that a child has knowledge of both sides of the relation. The hypothesis, that the grammatical relation of subject reflects an aspect of children's innate capacities, therefore predicts that *ga* will appear in Izanami's speech before *wa*, since in the first case but not in the second, a child has prior information about the deep-structure half of the relation, and so need discover only the surface-structure half in order to relate the two.

The facts are as follows. In the first 8 hours of recorded speech, Izanami used the postposition *ga* 75 times. It was never incorrectly used, and it was almost always present when called for, given the content of what was said. *Wa*, in contrast, was used only 6 times, and, in all except one of these occurrences, appeared with the same word. There were many, many contexts calling for *wa* into which Izanami placed nothing at all.

It seems clear that Izanami understands the use of *ga* but does not understand the use of *wa*. We could conclude at this point that she has available the grammatical relation of subject, except for one puzzling fact. The distributional identity of *wa* and *ga* in the surface structure of Japanese sentences, which made this natural experiment possible, should also have confused Izanami. Why did she not formulate two transformations--one for *wa* and one for *ga*, *relating both postpositions to the underlying subject of sentences?* The puzzle only deepens when we take into account the fact that *wa* was used twice as frequently as was *ga* in the speech of Izanami's mother, a difference that should have misled the child--causing her erroneously to choose *wa* as the postposition to be related to the grammatical subject. However, Izanami was not misled, and understanding why provides further insight into the capacities of children for language. *Wa* and *ga*, although distributionally identical, are used by adults under different semantic circumstances, and Izanami, by virtue of her linguistic capacities, was sensitive to only one of these. By distinguishing the situations appropriate to *wa* and *ga*, therefore, we can refine our conception of what these capacities are.

In what follows, I first try to provide some feeling for the distinction between *wa* and *ga* through the use of English examples, and then, following Kuroda (1965), try to characterize the distinction more explicitly.[1]

[1] I am indebted to my wife, Nobuko B. McNeill, for her absolutely invaluable assistance at this point.

A man-ga is standing on the corner.

The man-wa is standing on the corner.

Man-wa is mortal.

Cats-wa eat goldfish.

The cat-ga is eating the goldfish.

Children-wa make too much noise.

Izanami-ga makes too much noise.

This-wa is a digital computer.

According to Kuroda (1965), the subjects of sentences that state general truths, subjects that have attributes given to them by their predicates, subjects that function like the logical premises of judgments, and words like *this* and *that* when they are used in definitions, all take *wa*. Kuroda calls this usage *predicational judgement*. Quite often, it can be translated into English with the help of the expression *as for*.... Thus, the examples given above can all be rendered:

As for that man, he is standing on the corner.

As for man, he is mortal.

As for cats, they eat fish.

As for this, it is a digital computer.

In each case, an attribute--standing on the corner, mortality, fish-eating, computerhood--is judged applicable to the subject of the sentence, and so the subject takes *wa*.

The postposition *ga*, in contrast, is used for any linkage between subject and predicate that does not involve attribution. Rather than the predicate of a sentence being a property attributed to the subject, the subject and predicate stand in an equal relation to one another, in which the predicate is not construed as an inherent property of the subject, as it is in *wa*. There is no standard English translation for *ga*, although the progressive aspect of verbs, --*ing*, comes fairly close. One can say, for example, *some man is standing on the corner*, or the *cat is eating the gold fish*, for most Japanese sentences with *ga*.

The fact, therefore, that Izanami acquired the transformation for *ga* and failed to acquire the transformation for *wa* indicates that she is grammatically sensitive only to those conditions that prevail under non-attributive description, and not to those that prevail under attribution. The same may be true for children acquiring English--progressive aspect was the first verb-inflection learned by the children studied by Bellugi (1964). This result is interesting for several reasons.

One of the examples of *wa* given above was a definition: *this-wa is a digital computer*. Izanami's mother uses *wa* in this way often: *this is a crane*, *that is a tape recorder*. Another example of *wa* involved the establishment of a general relation: *man-ga is mortal*, and Izanami's mother uses *wa* in this way, too: *grandmother lives in Kyoto*, *daddy is big*. We can be absolutely certain that Izanami attends to these statements. They are the only way in which she obtains new vocabulary and new information about the world, and there is little doubt that she acquires both. She knows what a tape recorder is, she knows that her grandmother lives in Kyoto, and she understands that her father is large.

Such observations are relevant to a theory of language development favored by certain behaviorists, which runs something like the following. Children begin to learn the names of objects, events, qualities, etc., at around 1 year of age. They continue uttering such isolated names for 6 to 8 months, adding new words all the while, but never uttering two or more names at once. Eventually, however, children reach a point where they know the names of certain objects, events, or qualities that appear together in stable relations within the environment around them. Children then combine two old names to make a new name, and sentences like *doggie bite* and *baby sleep* are the result. This is one view of the origin of grammar in children, and Izanami's evidence refutes it.

If children do anything like what this theory claims, Izanami's postposition would have to have been *wa* and not *ga*. In parental speech, only *wa* is used to refer to such stable relations. Thus, Izanami, does precisely the opposite from what the theory, that grammar arises from naming, would predict. There is a further implication. Since Izanami, like all children, does name things and events a great deal, we must conclude from the absence of *wa* and the presence of *ga* that the cognitive achievements in support of naming and those in support of grammatical development are completely separate in children. Even when a language provides a grammatical form associated with naming, children ignore it in their early sentences.

The basic grammatical relation of subject corresponds to the concept of momentary linkage. One would say that the reason all languages have grammatical subjects and predicates is because all children attempt--from the outset--to combine word meanings in a very definite way. Only those combinations that are temporary and non-attributional are grammatically significant. Others belong in a different domain, in the domain of names. Perhaps there is some biological advantage to this arrangement. If attributional relations had grammatical significance, grammar could develop only in proportion to a child's accumulation of general knowledge. Or, if not that, then every combination a child willy-nilly produced would have to be stored in memory as permanent information about the world. But neither of these is the case. Instead, a child treats permanent information and the ways of combining such information separately.

Japanese and English are radically different languages. Yet, children exposed to them do the same things at points where linguistic theory claims they should. To Izanami and Adam, I can add a third child whose mother tongue was Russian (Slobin, 1966), and here, too, the basic grammatical relations were honored in earliest speech. Similar observations have been made of children exposed to French, German, Serbian and Bulgarian. So the hypothesis with which we began, that the theory of grammar reflects children's capacity for grammar, gains a measure of empirical support.

I would like to return briefly to the question of disadvantaged children and their linguistic development. I speak as a novice, totally unacquainted with the problems of teaching underprivileged children, and with no idea, aside from casual observation, of what their language is truly like. Such caveats not withstanding, I would like to suggest the possibility that the differences in dialect associated with differences in socio-economic status are, cognitively speaking, marginal and slight. One grammar is as good as another, and one grammar is as demanding as another when being learned. This is true across languages. It would be remarkable indeed if it were not true within languages. Problems of prejudice (such as dialect rejection) aside, it is at least possible that there are *not important cognitive deficits associated with the* language of disadvantaged children. The step of examining their language may be, in this case, a step in the wrong direction, for the deficit may exist elsewhere.

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John's commentary on McNeill's paper

I would like to start my discussion by asking what the biological justification for the differential universality of linguistic proficiency vs. cognitive proficiency might be? If you postulate that there is a linguistic proficiency which is universal, but a cognitive proficiency which is not, are we returning, I wonder, to the questionable implication that all people can talk, but certain individuals might not be able to think. I am exaggerating Dr. McNeill's position, of course, in order to emphasize the dangers inherent in differentiating linguistic competence from cognitive competence, when that particular distinction is pushed to the extreme. In doing so, I am not assuming that either Dr. McNeill or Dr. Chomsky ever so pushes it.

It appears plausible that the most rudimentary forms of language (at the phonemic, morphemic and syntactical levels) appear in highly similar fashions in the repertoires of young children. This uniformity might, as Lenneberg and Chomsky and McNeill have argued, be the result of "an innately mapped program for behavior." In the first year of life there are indeed, certain occurrences of sounds which are not specifically within the hearing environment of the child at the time that he is learning to speak; but around the age of one, a phonetic shift toward the phonemes of the particular language which is dominant in the child's environment takes place. Similarly we might postulate processes at other linguistic levels where rudimentary communality in forms does appear early in language acquisition, but where enormous variations in the range and adequacy of language forms appear in the children's verbal repertoire soon after the rudimentary forms have been mastered.

The sharp divergencies which have been noted--whether by Irwin in phonemic production at the age of 18 months between middle class and lower class children, or by McCarthy between only children and twins--can be traced only to certain antecedent environmental conditions. If children hear and use language in highly restricted learning situations, whether imposed by poverty or other confining circumstances, then their subsequent language skills will reveal a slower rate of acquisition. This is where our evidence lies, as well as, (and more significantly) in the limitation in diversity of language use, as Dr. Hymes has discussed so eloquently this morning. Such a poverty in functional diversity may be expressed by a limited ability to convey or control emotions verbally, to plan play or activities by means of words, or even, in certain instances (and this is equally prevalent in lower class and middle class environments) to engage effectively in interpersonal communication.

This kind of language utilization must be distinguished from the utilization of that language which has occurred with very high frequency in the child's hearing environment and is really overlearned--these are the forms which occur frequently in the child's life. That is, it is quite likely that children will be exposed to certain uniformly occurring rudimentary syntacical forms in any social class. But the very broad hypothesis that I am putting forth is that significant variations between children raised in different environmental conditions, are more likely to be revealed in the acquisition of language beyond the rudimentary mastery of phonetic and syntactic forms.

The whole question of a lesser variation between social classes at the grammatical level and a greater variation at the functional level and in terms of rate make an enormous amount of sense. One of the problems in a confrontation between the biological and environmental positions is that our data come from different age ranges. Most of the data of the Chomsky school come from ages 1 to 2½. Most of the data from the more environmentally oriented school of those involved with disadvantaged children, come from ages 2½ to 7 or 10. Among these older children the significant point is that there are variations in syntactic forms between lower class and middle class children, and the variations are greatest in the syntactic forms normally acquired later in age. It is in the use of adverbial phrases, for instance, that we find significant differences between middle class and lower class children, much more so than in noun phrases.

To sum up then, one of the areas in which we do have significant differences between middle class and lower class children is in rate of language acquisition. A second area in which significant differences occur is in certain syntactical patterns, but not the simplest ones. Now I'm not here speaking about certain recurring features of the dialect, tense forms, and so on, but rather of areas of difference which may be more related to a grammatical theory. And thirdly, and most importantly I think, those differences which are of major concern to us, differences in the utilization of language, and in communicative competence relating to cognition (often confused with that communicative competence which relates to social effectiveness). I think it is important not to confuse these, because of we really accept a theory of the kind that Hymes puts forth about use being in the competence column, then we might well be able to look more systematically at differences if we do not try to combine communicative competence into a simgle unique universal which we measure by the mastery of certain syntactical patterns. It is this particular point of the McNeill presentation that I am most unhappy about.

You cannot put everything relating to language competence under the heading of a single language universal, evidenced by the stringing of certain words into a pattern at the age of two and then argue that language is biologically the property of all individuals if you then argue that you now have to divide language and cognition, because here we have evidence that language is biologically available and there we have evidence of cognitive differences and hence they must come from two different areas. I myself am deeply convinced that language and cognition are interlocking processes where language is a significant tool for the development of concepts and where concepts contribute to the more effective utilization of language. They can only be seen as coming from two different areas if we use the evidence related to rudimentary syntactical patterns as our major argument concerning all the different things that we usually mean when we speak of children's capacity to utilize words, sentences and longer passages. These cannot in any sense, be analyzed only in terms of grammatical universals. Moreover, in terms of empirical evidence, we know thus far only about grammatical universals at very, very early ages. Of course one can't really blame the people whose empirical work is restricted to this age level, because the moment that the child engages in a more complete and more effective communicative pattern, the types of analysis, the searches for universals become increasingly difficult if not impossible. This is why we have to, in our empirical work, both develop and follow through on a much more carefully differentiated analytical model which will allow us to make specific statements concerning differences between classes, and differences between different individuals raised under varying social conditions as related to levels of linguistic analysis, levels of analysis of communicative competence, and levels of analysis in terms of social role and status. If we do not follow that kind of an analytical model, I think we will get into senseless arguments.

Discussion following John's Commentary on McNeill's paper

Sarles: Part of my confusion regarding the LAD model is that it seems to present a notion of a very passive individual existing all by himself on whom we put a whole bunch of rubber stamps. I don't think children are like that. Part of the earlier structural models they may have to have is something that makes them go back to the environment and actually elicit information from their parents and other people. The child at two can get away with an awful lot of things that the child at four cannot get away with because his parents perceive him as a different kind of object and the child himself perceives himself as different. To conceive of a passive individual going through life becoming more and more rational and more and more correct seems like a great oversimplification of all the irrational things that are going on which make him become a grown-up human being.

McNeill: I can't imagine how you got the impression that LAD is passive--it's obviously just the opposite kind of theory. The theory of language acquisition here is one of hypothesis formulation and testing. If you interpret literally what I've said this morning there's a clear absurdity. It would be consistent with what I've said so far to predict that exposure to English leads to competence in Japanese--and vice versa, on some random basis--which clearly is not true. What I've omitted entirely from mentioning is how children go about acquiring transformations which will yield the distinctions among the two languages. I omitted that a) because I didn't have enough time, and b) because no one really knows anyway.

Sapon: I'd like to ask a pointed question. I'm perfectly willing to accept the fact that you account for the acquisition of language by children simply because they happen to be of the homo sapiens variety. But how do you account for those children who do not acquire language? My concern here is that your basic position is that all that is requisite for a child to become an appropriately functioning member of a linguistic community is that he be a) a human being, and b) that he be exposed to language with no further specification as to the nature of exposure. And the issue here is how do we go about modifying patterns where children do not indeed demonstrate this supposed natural behavior. I'd like to pick up Dr. John's suggestion to the effect that there is a very solid implication to the effect that those children who do not learn to speak either accurately or appropriately are somehow less than human and our intervention procedures must then concentrate on humanizing them.

McNeill: I was shocked by the comment because it seems to me it implies a latent assumption that the only language worth acquiring is middle class English.

John: I did not say that. What I said was that the assumption that linguistic competence is universal and biologically pre-determined, and that cognitive competence is not biologically pre-determined is questionable. It brings up all kinds of very serious questions as to how cognitive competence is acquired as contrasted with linguistic competence which is inherited.

McNeill: Well, linguistic competence clearly in not inherited. What is inherited is a capacity to acquire language. Linguistic competence is the result of an interaction between environment and that capacity. I would like to clarify my point of view because it seems to me there may be some difference in research strategy lurking around here. We can only gain by being analytic in attempting to understand problems of cognitive development--it does not increase our understanding to systematically obscure distinctions that can defensibly be drawn. It seems to me it would be helpful to know that all children, regardless of their social backgrounds have an inborn capacity to acquire any linguistic system that conforms to linguistic theory and that this will happen automatically and to a level of proficiency that is perfect and optimum. Thus you can narrow down those areas of cognition where problems may exist for which you may eventually hope to achieve some remedy. If we define linguistic as a capacity to formulate a grammatical system and to produce utterances consistent with that grammatical system, then my suggestion was that the difficulty confronted by someone from an impoverished background may not be linguistic, but rather may take quite different forms--e.g., a completely non-linguistic tendency to exploit grammar for cognitive purposes, or a completely non-linguistic ability to communicate with a middle class teacher in a school.

Sapon: But I want to insist that the title of your paper was "How To Learn a First Language" and I cannot find anything in what you said that has the effect of a "how to".

McNeill: If you accept the gist of my talk then you have learned that the problem of acquiring a language does not exist. It seems to me there is some advantage in formulating the issues. It seems to me there is no merit in blurring such distinctions and calling the whole thing linguistic.

Gumperz: May I say something to tie Mr. Hymes and Mr. McNeill's presentations together? I think one of the implications of what Mr. Hymes has said is that our notion of what is linguistic needs expanding, and that although we don't yet have the explicit techniques for the study of socio-linguistic phenomena that we have for the study of syntax, that it is possible to deal with these phenomena in the same way. Implied in the notion of linguistic is the idea that this is somehow unconscious behavior, patterned behavior, behavior that connot simply be changed by telling the person "change you habits". I think what Mr. Hymes has said is that the same process of hypothesis formation, the same patterning holds in the socio-linguistic realm, and that this is what we need to look for. Now as a linguist I've watched the notion of grammar develop, and in the field of South Asian languages in which I've done most of my work I have seen a radical change in the notion of, for example, what constitutes deep structure and what constitutes surface structure. These notions are going to continue to change. In other words the concepts of surface structure and deep structure are themselves in the process of development--why should we set artificial bounds on what is linguistics.

McNeill: I understood Dell Hymes to be suggesting, as you said, that there exist large areas of linguistic activity that may present a distinction between competence and performance, just as the distinction has been honored within the domain of syntax. I'm not sure that he went so far as to say that the distinction between socio-linguistic competence, on the one hand, and syntactic competence on the other, should be abandoned. It's possible that by calling both of them competence we'll accidentally overlook the fact that they may conceivably be quite different.

Now there's another distinction here between the kind of processing that goes on in producing grammatical sentences or in understanding grammatical sentences and the sort of processing that goes on in deciding what sort of sentence to produce in a given situation. That processing may be all of a piece, and the proper psychological theory may have to incorporate information about all of them. That in itself, however, doesn't argue for a unified linguistic theory to encompass both. If you accept the distinction between competence and performance at all, you have left yourself open to the possibility that there will be describably different sets of competences all of which feed into one underlying performance mechanism; and also the opposite, perhaps one underlying competence feeds into a variety of different performance mechanisms.

Gumperz: In transformational theory, the implication is that the central core is syntax, but there is another kind of theory, something which has been proposed but much less explicitly stated in which you have a semantics, a syntax and phonology, and that the relationship between these and the processing mechanism is one of realization. Now I think there's difference between the notion of realization and the notion of transformation. Transformation is something which operates within this theoretical framework of competence, whereas realization rules somehow lead from the cognitive down to the actual performance. I think they're two alternate models and I don't believe that Chomsky is committed to the transformational model. So I think that it's premature to define linguistics in terms of this model, and by doing so I think we're committing ourselves to a theoretical position which precludes our getting data. By talking in terms of a realization model, we're not as explicit as we are in stating the transformation model, but I think we can incorporate much of the work that's been done into this kind of a framework.

McNeill: A theory of language acquisition is logically secondary to a theory of linguistics, since you can only raise the question of language acquisition if you are able to define the linguistic system to be acquired. So one properly should speak of a theory for the acquisition of a stratificational grammar, and another theory for the acquisition of a transformational grammar, and so forth--and what I've been concerned with is the acquisition of a transformational grammar.

Sapon: I would like to ask what may seem like an absurd and essentially redundant question. What's the definition of "linguistic" controlling the discussion going on here this afternoon? You are apparently operating with a definition of "linguistic," and apparently there are some of the members of the audience operating on the basis of the same sort of definition, but I think there are number of members of the audience who are not in phase with the definition that seems to be involved.

McNeill: A language is defined in the theory of grammar--this is the definition I've been using--and to describe to you then what I mean by linguistic I would have to be able to describe to you the theory of grammar as it now exists--which I'm not about to do. But it would include such things as a specification that all languages are transformational.

Sapon: I'm sorry to carp, but that seems to me to be very circular. Now you have made a clear cut dichotomy between linguistic concerns and cognitive concerns. Since we get insight and access to cognitive processes by verbal means, we now have the interesting problem of dividing what is linguistic from what is cognitive. I really think I must press for a clarification of what is excluded from the definition of linguistic. You said earlier in reference to the problem we are concerned with that it was not a linguistic problem but another kind of problem. There's a very narrow definition of linguistic going on here.

McNeill: That's right. It seems to me that a scientific definition should be made as narrow as possible; and all I can tell you is that the definition of language that is embodied in linguistic theory is a) under development, and b) can be modified in various ways. A certain constraint involved in linguistic theory such as "all grammars must be transformational" can be rejected by showing that a transformational solution for the structure of some language is inadequate on various grounds--so then that aspect of the definition of language could be rejected.

Sapon: Where, for example, do communicative processes whereby human beings interact socially, such as kinesic behaviors, fit into the definition of linguistic?

McNeill: Are we defining linguistics or language?

Sapon: Both, I presume.

McNeill: I suppose that's a mistake.

Sapon: Could you rectify the mistake?

McNeill: Well, one is a science, and one is a particular, hopefully well-defined subject matter. Linguistics might be engaged with all sorts of things, among them kinesics, paralinguistics, syntax, phonology, etc. I don't think it's as crucial to define linguistics as it is to define language.

Sapon: I think it's crucial to define both since the problems we confront in this session have to do with tabbing what concerns us as linguistic or non-linguistic and I have the feeling I'm trying to catch a greased marchmallow. We've got the problem here seen as partly linguistic and partly non-linguistic and I'd like the audience to get some notion as to where the boundaries are.

McNeill: When I say the problems of disadvantaged children are not linguistic, that statement is based exactly on the definition of language embodied in linguistic theory. That is, what I asserted is that children of every socio-economic class possess a transformational grammar by the time they're five years of age and that they have the capacity to use this grammar with equal efficiency.

And within the context of linguistic theory, supplemented by an as yet strictly promissary psycho-linguistic theory that would cover the performance aspect of language, that is to assert that there are no real psycho-linguistic differences. Now obviously I've defined implicitly a certain subject matter, and so long as the definition is borne in mind, it seems to me that nothing is lost and a great deal is gained in that we can now define another subject matter, namely, disadvantaged children. Whereas before two subject maters were lumped together and a critical distinction was lost, leading us to believe that there must be some differences within the linguistic psycho-linguistic system, as well as differences elsewhere. It seems to me it's relevant to know whether or not this is true. That's the reason I would like to formulate the question in this way.

Cazden: I would like to ask you where you think the differences do reside, to specify what you think they might be and what areas you think they might be in. You've said where you think they don't reside.

McNeill: I really haven't the faintest idea. The one thing I suggested was that Brunnerian symbolic representation may be acquired through certain special kinds of social interactions peculiar to the middle class. Let's pretend along with Brunner that symbolic representation is a handy thing to know how to do, so that the tendency to use language for ratiocinative purposes may be a non-linguistic attribute of advantaged children. It seems to me you can conceive of this being done with any kind of linguistic system, since it depends not on the linguistic system, but rather on certain kinds of preliminary social conditions not ordinarily met in the lives of disadvantaged children. This is what I was imagining and it assumes a great deal--it assumes, for example, that symbolic representation doesn't involve an actual linguistic process so much as something that's done with a linguistic process.

John: I think that the major problem here is what we are defining as language, and I think that all of our differences relate to that question. The fact is that there are differences in language behavior between social classes, and these differences appear as soon as the most rudimentary forms of phonology and syntax have been mastered (actually this too is an assumption because we really don't yet have adequate longitudinal data showing at what point significant differences emerge). We know that at the age of six there are differences, and some of us call them significant and others call them irrelevant, cognitive or surface structure differences. Now it seems to me that unless we develop some kind of a working definition of what we mean by language, first, and then disadvantaged children, second, we will go on having somewhat pointless arguments. If we speak of language as that body of conventionalized sounds which have a shared meaning from speaker to recepient of language as this develops during growth, which might be one way of looking at it from a very simple psychological point of view, this would be just as inadequate a definition as to speak of language in terms of grammatical theory only, leaving out the social relationships and the social context in which language is learned and developed.

McNeill: Can I say just one thing. You can have the word "language" if you like, so long as we can agree that there is something that Noam Chomsky characterizes as syntax, something that he might characterize as phonology and semantics and in addition some as yet unknown and unformulated theory of psycho-linguistic performance that goes along with this; and then if you want to call all this plus some other things language, that's all right--we'll think up another term for what I just described, though in the context of generative grammar what I just described would be considered language plus a performance model, so the term language is even more restricted. All we really have to do is keep straight what we're talking about.

Osser: I'd like to comment on an earlier point you made about the equality of children from various socio-economic groups in terms of their linguistic performance. The implication of what you said is that if we as psychololists could devise a sufficient number of ingenious situations in which we could elicit speech from so-called disadvantaged children, we would find that the performance of these disadvantaged children is identical to that of advantaged children. You see I'm wondering at what point you can say there are significant differences in performance between advantaged and disadvantaged children--what kinds of evidence would you yourself accept?

McNeill: Well, I had in mind a fairly restricted set of phenomena and I can't define them very well, but it seems to me that such things as fluency of speech--the ability to produce words without significant interruption from other parts of his own linguistic system--might be equal. I say this because it is my impression that children appear to speak fluently from the point at which they are producing any sort of patterned speech whatsoever--there don't seem to be unusual latencies involved in producing two-word utterances in an 18-month old child. There don't seem to be an unusual number of interruptions and false starts; so even at the point where the grammatical system is extremely primitive and undeveloped, the performance system seems to be working at full efficiency. And if that's the case--if this performance system is largely automatic and efficient even at that stage, then it would seem to be incredible to assert that it must be just the opposite of this in the case of a disadvantaged child 3 or 4 years later.

It's difficult to be terribly explicit on this point because there isn't a theory of linguistic performance, so nobody has the foggiest idea what the parameters of that theory should be, hence it's a little difficult to know exactly what sorts of behavioral tests might be sensitive to this kind of linguistic performance.

John: But I think we should note that fluency and linguistic performance are the least significant differences between different social classes, so that we might be in agreement here, though I think the notion that we even have fluent speech at 1½ is questionable.

A Theory of Linguistic Performance

Wayne O'Neil

I come to this problem of language disadvantage with very little actual work in the field. Perhaps I differ from Mr. McNeill though, in that I am beginning to move into the field, so I cone for advice as much as to tell you what I think. In the fall of 1966 a number of us at Harvard-- referred to loosely as the school language group-- will begin work in Roxbury on a long range project: we will study the language of the disadvantaged, of their homes and of their schools. We want to characterize that in language they do and can do, what they hear and understand and what they can hear and understand-- with an emphasis on the can. Then on the basis of what we learn about their deprivation, we will want to develop and recommend two curricula: a school-wide, school-long language-based curriculum, on the one hand; and a teacher-training curriculum-- language study for all teachers-- on the other. Perhaps what we finally accomplish will have relevance only to the problems of urban America of the mid-20th cantury; yet I would like to believe that we are in fact addressing ourselves to a version of a quite general and universal problem that is somewhere in the middle of an aggravation scale. For ours is a good deal less overwhelming than the problem of, say, black Africa as it attempts to dislocate its people into English or French and into contact with technology and the 20th century. Yet certainly our problem is a great deal more aggravated than those of a self-satisfied middle-class white America. For we all lie far from full control of our language, and, thus, far from full understanding of ourselves and our world. The relevance of language study to education is in the help it gives toward reducing that distance first for those who need it most, and first for them because the aggravation of the situation allows us to see more clearly what is wrong.

In many ways our proposed study is an acting out of a good number of the points pursued at the First Working Conference on Language Development in Disadvantaged Children. I was struck by this as I read the summary of the proceedings a week or so ago, But was also struck (since we consider it crucial to pursue and emphasize linguistic theory) by the fact that "the majority view was expressed by Dr. Hymes who held that while we still need to work at formally characterizing the sentences and sequential patterns of languages, we must not get carried away with the ever-increasing precision of linguistic research technology." Mr. Stewart evidently demurred, and so do I. And in a recent article by one of Mr. Bernstein's associates-- Denis Lawton (who incidentally writes with the same deprived, passive, faceless style as Bernstein-- "it is hoped, it is thought, it is suggested"), he concludes: "In future work it is hoped that it will be possible to take advantage of the modern methods of linguistic analysis which are now being developed in this country"[1]

For before we set a martini with its sophisticated and supersensitive olive before our audience, we should have somewhat sophisticated theories of language and language use to set before our audience, theories that inform our data gathering, that give substance to our hunches. Very little can be accomplished otherwise though much is left undone even with the insights of theory, for our theories of language are not nearly so sophisticated as language. The dangers of the theory are then not in its being precise and technical, but in the limitation of its precision. It simply does not deal with all that is relevant to what we will speak about and *that* must be recognized.

But where it is relevant and informing, we cannot hesitate to be guided by the best of linguistic theory. For example, any theory of language forces us to distinguish sharply between two things often confused, between dialect and what we can call, following Bernstein[2], code-- or to use the terms of Chomsky[3] in their technical sense, between competence and performance, which are akin to Saussure's *langue* and *parole*. (It should be admitted, however, that these terms are not genuinely in such as easy one-one relationship, that Bernstein's *code* is a much larger term than Chomsky's *performance*, in fact includes it.). Mr. Hymes this morning gave a not precise enough characterization of the differences between competance and performance. I think this has led to a continued kind of confusion. Instead of defining performance, I think Mr. Hymes gave an instance of performance, which is quite different. Competence refers to the grammar, to the rules of various kinds that specify the sentences of the language. These are the rules that each of us has somehow internalized; performance has reference to how we put the grammar to use, to the complex set of constraints and conditions that lie on our use of the internalized grammar in actual speaking and hearing.[4]

Dialect differences are thus differences in grammar, in competence. Code differences are, in part at least, differences in language use, in performance. Significantly, Bernstein consistently maintains that he is concerned with "linguistic differences, other than dialect." He does not confuse the two, nor should we. Dialect differences are simply a natural result of the separation of the speaking community into separate areas; code differences are complexly a natural but vicious result of deprivation, dispossession, and denial. We should not speak of correcting dialect even though for some reasons known only to us we may want to change it. We do, however, want to enlarge, and in this sense correct, the code of the deprived; in order to enrich their language use one would like to, and have to repair *society*; short of that we can only ask how to repair education and the schools, how and what to teach in them. As a negative instance of what not to teach in them-- and as I learned just a few moments ago this is again a controversial point-- I am not convinced that we should be concerned at all to change dialects, to promote bi-dialectalism: to attempt to effect change *there* is hardly to help in the job of repairing society; it is in fact, in a way, giving into the petty prejudices of society, and it will require the expenditure of a fantastic amount of energy-- all of this for what I consider basically a rather questionable enterprise.

However, if one is going to attempt this, he should at least understand the complexity of the task. For if there is one thing linguistics has learned from the vain attempts of the last decade at machine translation, it is that stating the problem (e.g. that you want to translate arbitrary senternce *X* of language *A* into the corresponding sentence *Y* of language *B*) is quite different from offering a solution to the problem. For example, consider an attempt to change a prestige-less (thūwn) (tune and words like it) into a prestige-full (tyūwn) (I presume that no one but the wildest Anglophile would really want to foist this upon lower-class children, but it's the principle I'm after, not the specific example). Effecting this change entails more than giving some version of a direction like "palatalize all dentals followed by (ūw)", for this direction gives wrong answers as often as it gives right ones, leading to such barbarisms as (nyūwn nyūwz) *noon news*. Or one can simply give a complete list of words for which the new pronunciation is to be used-- which is simply to state the problem in an exhaustive way. However, both of these suggestions are correct in this sense, that they are suggestions that have relevance only on an abstract level of the language. The problem is to effect a simple change in pronunciation; the solution is however to effect a complex change in the abstract phonological representation of a whole list of items and to relax the conditions that lie on a phonological rule. Only when the complexity of the task involved is understood will simplistic notions of dialect change be avoided.

A grammar of a language is a complex deeply layered system and no one knows exactly how to reach into it to change this or that so that out in the surface of language the desired changes are made. To teach a second dialect is a special and complex case of teaching a second language-- complex because of the extent and specific nature of the common core of its two dialects. Since we clearly do not understand much of what goes on in second language learning, it is foolish to generalize from this lack of knowledge to second dialect learning and teaching.

Much more interesting and certainly more valuable is the study and change of performance. For only a richer performance can lead to a richer understanding of, a coping with, and perhaps a measure of influence over one's life. In a recent article Bernstein has commented upon the inaccessibility of psychiatric interviews for people speaking a lower class code.[5] This is certainly not to have a measure of control over one's life, not even to be able to understand one's basic internal problems. I am not naive enough or optimistic enough to believe that improved language performance will make a man employable, but I do believe that it will at least help him figure out why he is not employed.

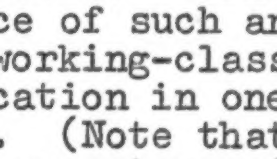
We thus want to characterize the language performance of the groups and individuals that we are concerned with. Our assumption is that in the constraints that lie on what the lower classes can say and understand are to be found the differences that we underatand as deprived. We assume further that there is essentially nothing very different in the grammar of deprived and non-deprived language. Notice now that there is a distinction between what Mr. McNeill said this morning and what I'm saying now. I'm agreeing with him to the extent that I believe that the grammars (the competence) are the same roughly from dialect to dialect, that there are superficial differences but no gross differences. I would tend to hypothesize that the differences lie on the performance side. To repeat, we assume that as between the grammar of the deprived and non-deprived one is not more primitive, less complex, etc. than the other. Indeed, we maintain that dialect differences are generally superficial in that they pertain to the finer details of pronunciation and inflection rather than to deep-lying differences of general significance. That is, all speakers of the dialects of American English share for all important purposes the same set of grammatical rules. What differs-- at least-- is their intelligence independent, socioeconomic class dependent, linguistic performance-- their varying facility in weaving the basic stuff of the language into complex utterances, the varying facility in deftly receiving and understanding complexly woven utterances.

For our studies then, we need to develop a narrow theory of performance. In what follows I will merely suggest the direction that our thinking has taken us so far. Typically we tend to talk about performance in statistical terms. Bernstein, for example, speaks of the statistical preponderance of such and such a construction in middle-class as opposed to working-class speech, for example that there is more complex verb modification in one than in the other, more complex noun modification, etc. (Note that it makes no sense to speak comparatively of linguistic competence in statistic terms, except, for example, to say that one grammar is longer than another-- whatever might be the consequences of that, although I have a tendency to believe that English has a more complex grammar than some other languages I've worked with. We cannot speak of the average length of sentences in grammar, of the maximum depth of embedding, of the maximum number of times a recursive rule may recur. The answer to these is always the same: infinity.)

Consider, for example, a type of sentence that has been discussed by Chomsky and Miller together and separately.

(1) the boy whom the girl loved saved the day.

It is a rather ordinary sentence except that it is syntactically characterized by a kind of self-embedding known as nesting. That is, within a sentence of a certain type we have embedded a sentence of the same type.



The rules of grammar must be written so as to allow this perfectly grammatical sentence. But in writing the grammar so, we immediately allow not only the grammatical and acceptable:

(1) the boy whom the girl loved saved the day.

but also the grammatical but for most people unacceptable:

(2) the boy whom the girl that the dog nuzzled loved saved the day.

and the grammatical but totally unacceptable:

(3) the boy whom the girl that the dog which the fleas bit nuzzled loved saved the day.

Presumably there is a general performance limitation at work: to quote Chomsky, "to account for the greater unacceptability of self-embedding (assuming this to be a fact), we must add other conditions on the perceptual device beyond mere limitation of memory. We might assume, for example, that the perceptual device has a stock of analytic procedures available to it, one corresponding to each kind of phrase, and that it is organized in such a way that it is unable (or finds it difficult) to utilize a procedure φ while it is in the course of executing φ" (Aspects, 14).

We, of course, are not in principle concerned with general performance limitations, for we are sure that the troubles of the disadvantaged will start long before those of Chomsky and Miller. But still there is something in this example that is worth pursuing. For if it is true that human beings are so organized as to find it difficult or impossible in some maximum sense to utilize a routine while in the process of executing that same routine, might it not be possible that a generalization of this principle will hold in language disadvantagedness, that is that it will be characterized by a difficulty or an inability in utilizing a procedure while in the process of executing the same or merely similar procedure. A limitation of this sort, one rather severely short of the general performance constraint, would explain the paucity of complex modification as well as the general absence of complex subordination that has been reported in the literature.

We need, then, to develop some understanding of these constraints. We also need to get at the related notion of syntactic complexity, in terms of which performance constraints will presumably be defined. For we would like to be able to make a general statement about the limits of syntactic complexity that the people we are examining can reach to both in speaking and listening. It is important, further to find out exactly what level of complexity they are asked to attend to.

Surely syntactic complexity must depend upon a number of factors. Again we can do no better than to begin with Miller and Chomsky and their notion of node-terminal node ratio. For example, given a phrase like "the long hot summer", we have a node-terminal node ratio of 5/4-- as the ratio approaches one (it can, however, never be one) the structure is said (all other things being equal) to be simpler.[6] That is on this measure "the long hot summer" will be said to be less complex than "the long summer that was hot" with a node-terminal node ratio of 9/6 and both of these less complex than "the summer that was long and hot" (11/7), and these less complex than "the summer which was hot and which was long" (15/9). Complexity is obviously a function of the extent to which modifying elements are embedded-- sentences with single word or phrase modifiers being less complex than sentences with clausal modifiers: "the tall man" vs. "the man who is tall"; "the man from Chicago" vs. "the man who is from Chicago."

But notice the restriction "all other things being equal." For "the boy" (3/2) and " the long summer that was hot" (9/6) are of quite different orders of complexity though they have N-TN ratios of the same order. The second is, of course, longer than the other, but than's not enough. Much more important is the fact that the latter phrase has embedded material in it-- constituents deriving from underlying embedded sentences. So another measure of complexity is that related to amount of embedded material. Thus "the tall boy" is less complex than "the tall boy from Chicago" on this measure and less complex than "the boy who is tall" on the other and less complex than "the boy who is tall and who is from Chicago" on both counts.

It will, I believe, be possible to make more precise these two dimensions of complexity. But there are further and more bothersome areas of syntax and complexity.7 For example, the structures:

(1) because he stayed I left.

(2) the boy who is tall.

have the same N-TN ratio (8/5), the same number of underlying sentences, but yet I presume that we intuitively feel the first to be more complex than the second. This is certainly because embedding in the first is a far different thing from the embedding in the second. For in the second there is a simple identity of nouns controlling the embedding; in the first there is a logical relationship at stake. If, for example, we are to make a more complex sentence out of the two sentences:

(1) the boy is here.
(2) the boy is tall.

we need only mark the identity of the subject of the two in order to carry out an acceptable embedding, just as in receiving and understanding:

(3) the boy who is tall is here.

we need only to understand the referent of who. But in sentences tied together by because, if, then, etc., there must (in order for them to be acceptable) be an understanding of causation, conditions, consequences-- in short, of the relationship of event and idea. "All other things being equal" has thus led us on to another dimension of complexity: we must distinguish between simple and complex subordination. The two types of subordination are clearly distinguished in grammar, in the kind of constraints that run over from one to another. That is we could expect absence or misuse and misunderstanding of the latter without attendant difficulties in the former, though the reverse would hardly be the case.

We have then begun to develop three measures of complexity; there is of course much more to talk about, but this is enough to give you some idea of the problems involved, of the solutions sought. Our most egregious technical problem is in another direction: how do we get the data we seek? At least half of the answer is relatively simple: for insofar as we are concerned with what the child typically hears, we merely listen to those who speak to him-- to his teachers (especially), to his peers, to his elders. But insofar as we are interested in what he _can do_, we face the, at the moment, insoluble problem of how to draw his language performance out to its full extent. We do not want to hear what he typically does-- for presumably it will not differ from what he typically hears at home and at play, but we do want to know what he is capable of. This we must somehow torture out of him. Here, of course, linguistic theory has nothing to say. There seems to be no clear and neat design available.

But even after we have developed and supported empirically our theory of performance, our notions of complexity, we will not have begun to exhaustively characterize the lower-class code. For grammar reaches only to sentences, not beyond: we have as yet no revealing way of talking about the larger patterns of discourse, any very interesting way of characterizing types of discourse. For example, all we can do is repeat, if it is true, Bernstein's observation that the working-class (a term often inappropriate for the people we will work with) narrates and describes only, while in addition the middle-class reflects and abstracts. Thus it uses many more _you's_ and _they's_ than it does _I's_. Our theories then can carry us only so far until finally we must revert back to precise and then, I presume, anecdotal observation.

Footnotes

1. _Language and Speech_ 6: 142, 1963.

2. For a good summary of Bernstein's work and for a complete bibliography of his work, see Basil Bernstein, "A sociolinguistic approach to social learning" in J. Gould, Ed. _Social Science Survey_, Pelican Book, 1965.

3. See Noam Chomsky, _Aspects of the Theory of Syntax_, M. I.T. Press, 1965.

4. Furthermore, there would be absolute performance limitation by virtue of the structure of the human organism and relative limitation by virtue of the structure of society. It is limitations of the latter sort that we are dealing with here.

5. "Social class, speech systems and psycho-therapy." _British Journal of Sociology_ 15: 54-64, 1964.

7

6. To arrive at a node-terminal node ratio we need to work from a tree diagram of the phrase or sentence in question. Consider the phrase "the long, hot summer":

noun phrase

determiner adjective adjective noun
the long hot summer

where there is one more node (noun phrase) in the tree than there is total number of nodes.

7. In my thinking about the following structures, I have profited from discussions with Mr. John C. Mellon of Harvard University.

Sapon on O'Neill

I jotted down a number of points that strike me as being particularly interesting to talk about and they fall into several categories. Perhaps I can segment my remarks.

There is a fundamental problem of scientific procedure and the building of theories that I think we have to confront. I have long had the feeling that it is beholden upon the generators of a theory that the theory contain within it germs, at least, of a direction for the empirical testing thereof. It is somehow or other an imposition on the academic community to say, "Here is my theory. It was born full-blown from some Junoesque forehead. It is up to you dirty-handed empiricists to disprove it, but until you do so it is accepted as God-given truth." I think that we have been seriously imposed upon today in this specific regard. We have been promised, for example, in an earlier paper in a similar vein, instructions on how to learn a first language. I got from it no glimmerings of a program that would help me facilitate or accellerate the learning of a first language for a child in some way other than merely having him be blessed to be born human and be further blessed by being born into a community that used language.

The paper we just heard, talks about the applications of linguistic theory to the improvement and general amelioration of the language disadvantageness of a sub-set of our population. I listened very, very carefully to the answers that I hoped we would get. This may be defective listening or defective comprehension on my part, but I couldn't find the answers. Instead I heard a kind of voice of doom. I heard, for example, about an insoluble problem. The problem is defined as insoluble to begin with. So we now address ourselves to a problem defined as insoluble. I find this a disturbing way to begin my scientific endeavors.

I am very much concerned, for example, with the driving back into inaccessible regions of an organism, namely into his God-given nervous system, the explanation of his behavior and the explanation for his non-behaviors when they do take place. We talked about such notions as internalized rules. Now as I follow the logic of it we have a kind of elegant circularity-- a closed corkscrew that reads like so: people talk grammatically, ergo they have a grammar. This represents a new kind of scientific discipline which I would like to call ergotism. We know people talk grammatically because when we apply the efforts of a trained linguist we find that he is indeed able to make an economical and elegant and parsimonious description of what he does. Now this is our definition of grammar.

In one sense, of course, I realize this is a vast oversimplification and a leapfrogging over the issue of grammar. But we do indeed have grammar, identified by grammarians, and therefore we have people who (and this is again a quote) "have a grammar." There is an interesting leap that comes at this point: since the only grammar that the linguist witnesses is the grammar that's externalized, and since the speaker of the language must indeed have a grammar and it's not externalized like the linguists'-- we have another ergo-- if it's not external-- ergo, it is internal.

There's a further shift here about not internal grammars, but rather internalized grammars. An internalized grammar would seem to be one that came to be internal. This I see as a leap of several light years from a logical point of view, that is, to start talking now as if the internalized grammar was now empirically demonstrated fact so that we base all our theoretical discussions and our questions here on application of the assumed linguistic theory to the problem of modifying language behavior. Naturally the conslusion is that this is an insoluble problem, since there is nothing in this kind of theoretical approach that seems to suggest where our first step might be.

Now it is a relatively common thing for scientists attempting to gain theoretical insight into some phenomenon or other to say: this organism behaves "as if" the following processes are involved. As long as the "as if" is recongized as a kind of ever present caveat, I think we're fairly safe. But we make what I consider this light year logical or intuitive jump when the "as if" is now forgotten and we proceed to assume that we have described the internal processes of the organism.

Now rule-organized behavior is not exactly a new notion. Human beings engage in all sorts of vastly complex, highly patterned behaviors that are clearly non-linguistic. It is intriguing that no where on the horizon have we had any suggestion about the internalization of tennis playing or the internalization of piano playing or the internalization of double-entry bookkeeping. Language seems to have been sorted out as a very special and unique kind of human behavior, one surrounded by an aura of mystique represented by the statements about the biological foundations of language which celebrate the reverential law of human uniqueness. The question is how do we get at this internalized grammar? Surface structure has been spoken of generally today in depreciatory terms. Such and such a thing is mere surface structure, or such and such a thing is only surface structure. What really counts is the deep structure. And the deep structure, to quote again from an abundant literature is "fantastically complex." This is another way of saying that it is, in essence, an insoluble mystery, something that will forever remain inaccessible to us. We must get at it inferentially with what I consider an impressive number of logical steps in between.

Now I'm very anxious to get my hands on the kind of behavior in question, and I'm inclined to think that I have touched something that's relevant to the behavior when I can effect some changes in it. When I look into this theoretical position to seek a handle by which to move an organism's behavior, I find it singularly absent. The question of competence is another one of these fictional mystiques arrived at by logical inference, by an "as if" without the quotation marks. Competence is described as being in essence the possession of an internalized grammar. If the internalized grammar is not reachable, then the competence cannot be reachable. How are we then going to effect changes in competence?

This is a serious question. We are not interested in mere performance. We are not interested in things that represent only surface structures. We really want to get at the deep underlying, fantastically complex internalized grammar which equals competence. There was another line in this afternoon's paper to the effect that "language is complex and deeply layered and no one knows how to change it." Yet we must change it-- this is part of our charge. If a child's capacity "innate capacity" here, or "capability" must be "tortured out of him", I would draw your attention to the fact that once it has been tortured out of him it is no longer capability of competence, but rather mere performance. There are strong philosophical issues that I think need to be confronted here, my dissatisfaction with which I think I'm conveying.

"The boy whom the girl loved saved the day," and I've marked three plusses because I'm afraid my competence is low and I'm unable to recall all the embeddings. We have an interesting destructive testing procedure which engineers use. They overload a bridge until it breaks. Now what is language? We struggled for a definition this morning and didn't seem to get very far, but I should like to offer at least one very, very simple, and perhaps even simple-minded, way of looking at language, and that is to say that language represents a kind of social interactive behavior, that language functions, when it functions, as social interactive behavior. When a behavior breaks down, when it no longer functions, we can no longer honestly say that it characterizes the behavior in question.

I can, by virtue of some grammatical rule system, concoct a sentence which someone's intuition tells him is grammatical, but which does not serve to control any other human being's behavior (whom by all other measures is a functioning speaker of the language). Then we have not indulged in a piece of destructive testing of the human being; instead what we've done is destructive testing of the theory.

We find that it is possible with this theory to construct non-language, and to construct non-language in a variety of interesting and nontrivial ways. What have we demonstrated by saying that we have an optimal length or a functioning length? What is the virtue, for example, of demonstrating syntactic complexity that goes beyond the kind of verbal behavior we're concerned with in the classroom. Somehow or other I had the sense that there was a virtue implied in the child's ability to "understand" sentences of increasing complexity. If value is indeed implied, I think one needs to justify and explain wherein lies the value.

Finally, I find prominently absent in this kind of theoretical approach the definition of some terms that are used in a non-technical sense. What does "understanding" mean? Does understanding mean the ability to recreate a sentence using a variant? Does understanding mean the ability to reproduce one complex as a string of simplexes? Does understanding mean the ability to produce a response in concord with the utterance that the student is responding to? What is the consequence of presenting the student with a sentence which elicits a blank stare? Is there something wrong with the student? There is a kind of classroom philosphy that assumes that the teacher is always right and if the student doesn't respond appropriately that the student is dumb, or unmotivated or unable in some way-- that the teacher's pronouncement is, by definition, a correct one. Well, I offer instead the notion that when an utterance is produced that is intuitively acceptable by a linguist's standard, but produces no meaningful behavior on the part of students, then what you have is an example of nonlanguage, which hardly seems to represent the open portal to the problem here of modifying the language behavior of the youngsters. And, as obviously you may gather, my notion of language behavior goes considerably beyond what is defined by transformational theory.

O'Neil, following Sapon's commentary

It is very obvious that Mr. Sapon has missed even the rather clear points that I thought I made. For example the set of sentences that I gave which I said were at points grammatical but unacceptable were not then removed to the classroom and talked about. I was not presenting those sentences as sentences that would be said in a classroom to defy some child to understand them. What I did was try to generalize from that principle into rather ordinary sentences like "the tall boy from Chicago was here," which might cause the same sort of problem. This is an empirical thing that has to be tested. Also I suggested that one perhaps should not change competence, and that in changing language I was hoping that the change would come not at the competence level, but at the performance level, which is, I presume, not inaccessible.

I think it is your understanding of science which is rather strange and not mine. I think that people in transformational grammar are in a very strong position from the point of view of the philosphy of science. A man like Galileo, for example, may have once in a while run out into a tower to drop a few weights off to fool the public, but when he revised and changed his theories he did them totally on internal evidence, with nothing to do with empirical pport. Theories can certainly precede empirical support. This is the only way in which I think we proceed soundly on a sensible basis. Certainly a man like Keppler just took observations and from them induced a theory which explained the data. Then he would go to the plants and see if indeed this was supported by further empirical evidence.

And I think it's true that people do learn other things by internalizing the rules. I remember a story that was once told me about a young boy who later became a very famous chess player. Having only watched his father play chess with friends, and never having played a game, he said to his father one day, "I think I can beat you," and sat down and beat him. Presumably he had internalized the rules by observing on the basis of data that came into him.

Finally, I think I qualified the "insoluble problems" with "for the moment." It's not that I stop working on them, say they're insoluble, step back and go home. It's an insoluble problem for the moment. I have no interesting suggestion that I'm willing to put forth at the time. That does not mean that it's true forever. I think it is possible to demonstrate this. I've done work in Scandinavian dialects-- specifically in the relationship between two languages, Foroese and Icelandic. They would be said by most people to be separate languages, but they have the strange characteristic of being one-way intelligible. That is, a Faro Islander can understand an Icelander, but an Icelander cannot understand a Faro Islander. We can stand back and be mystified. We can say "By God, this can't be true. We know that anyone who sits down and tries to understand the other guy can understand him." However, if we look at an independently motivated phonological section of each of the grammars, and here's where the differences lie between the languages, we'll find that Faroese and Icelandic share the same underlying abstract phonological representations and that they share for a good measure of the time the rules that realize these as output. And indeed, Icelandic is a step on the way toward becoming Faroese so that the Faro Islander can, in a sense, understand Icelandic because it's internal to his language. The Icelander cannot understand Faroese because it is external. And this is a demonstration of the importance of deep and surface on the level of phonology-- I think a very valuable one.

Sapon: There are other alternative explanations of one-way understanding. Spanish and Portuguese are another case in point. There are other easier demonstrable ways of explaining the fact that you have a one-way communication without talking about the fact that Portuguese represents to the Spaniard an internalized archaic form of the language.

O'Neil: Extra-linguistic ones?

Sapon: No. There are other linguistic explanations. There are other linguistic approaches, other than transformation theory-- that's a point that might have been overlooked this afternoon.

William Stewart

As the social scope of American education continues to broaden, and as the skills become more and more related to the practical demands of a complex society, American educators are finding themselves faced with a variety of new problems, some of which may assume sobering proportions of seriousness and complexity. Of course hindsight may tell us that many of these problems are not really new. What is new is the realization that there is, or always has been a problem, and the priority which society is now ready to assign to its solution. The topic of my paper is certainly an example of this.

There is all the evidence that we need that the majority of Negroes in the United States have behaved linguistically in ways which are clearly different from those characteristic of most American whites, just about as long as there have been Negroes and whites in America. Historical perspective alone is not much help in this case however. For while linguistic and cultural differences between Negro and white in America have existed for a long time, the practical problems which these differences could create are relatively new, arising only when the old social order which kept Negro and white separate and unequal began to break down.

In Europe, there have been dialect and cultural differences which surpass in both age and complexity those to be found in the United States. Yet the social order in Europe was such that it was always a primary task of education to cope with dialect differences. Since the standardized varieties of most European languages have been derived from or were based upnn one of a number of previously more or less autonomous related dialects, there has always been a tendency in Europe to look at dialect as an additive which, no less prestigeful than the standard language, still has a certain autonomy of structure and function. Consequently, recognition of the effect of dialect in teaching the standard language was almost always one of the informal pedagogical skills of the local school teacher, even when formal linguistic knowledge about the precise structure of the dialect was lacking.

In America, the dialect behavior of Negroes and other minority groups was seldom accorded such autonomy but this presented no real pedagogical problem since such groups were usually excluded from the pale of serious education. The result has been that little if any informal experience has been developed, within the American teaching tradition, for dealing with dialect problems. Now, with the dialect speaking groups suddenly included within the pale of American education, our teachers have a problem. And this problem goes beyond mere unfamiliarity with, say, the linguistic characteristics of non-standard Negro dialect. The teacher is often not even sure where dialect begins and other cultural differences or even pathological problems devolve.

A great rush has now started to help the American teacher to help the economically and culturally underprivileged child. Social actionists, educational specialists, psychologists, sociologists, anthropologists, speech and hearing specialists, linguists and even psychiatrists have volunteered to give aid and comfort to the teacher. But these specialists too are usually Americans and therefore not used to dealing with dialect differences as such. Consequently they have often found it difficult to sort out linguistically deviant behavior from deviant behavior with other causes.

I've seen cases, for example, where cultural stuttering was viewed as clinical stuttering, where the effect of cultural constraints on language was interpreted as an individual emotional problem and where dialect features such as the lack of copula or the interdental continuants were identified with developmental problems. Let me explain a little more what I mean. Cultural stuttering of the type common among lower class American Negroes and in some West African societies, where politeness or deference is shown by stuttering usually at the beginning of the utterance, is interpreted as the kind of stuttering derived from emotional and neurological disorders. The constraints on language usage that I'm thinking of are age-graded restraints or constraints related to some sort of social structure where it's not appropriate for one person to speak very much in the presence of the other. This is similar in effect to situations where someone is emotionally disturbed and for one reason or another also doesn't talk. However, in one case it's culture and in the other case it's individual. With the copula I'm referring to structures like "She a lady;" with the interdental fricatives I'm thinking of promumciations like 'nuffin' for nothing or better yet "breve" and "bref" for "breathe" and "breath". These correspond in some ways, or look like they're similar to developmental stages, because in a child's normal development not all phonemic distinctions in what eventually becomes the language he acquires are developed at the same time. Some phonemic distinctions develop early, others develop at later stages. For example "th" "thu" as sounds distinct from "fu", "vu" and other sounds are often learned much later than many of the other phonemes in English.

Now the fact that a dialect may lack a copula, and the fact that a dialect may lack "th-thu" sounds can often be confused with developmental problems where the child never acquires these sounds but stays with "fu-vu" or "sz" or something else. A situation where a child doesn't use a copula for dialect reasons can also be confused, by someone who doesn't know much about the nature of dialect behavior, with a situation where, again for developmental reasons, the language acquisition process stops in deviant individuals. Therefore, in diagnosing deviant verbal behavior, one must be able to distinguish individual deviation for physiological, neurological, emotional or other personal reasons from deviation within linguistic norms where verbal behavior in conformity with one linguistic norm may be susceptible to being considered deviant because language behavior is being judged in terms of another different linguistic norm. The distinction is crucial since quite different kinds of remedial techniques are required to affect physiological, emotional and cultural behavior.

Of course it would help if there were good descriptions of American Negro cultural behavior, linguistic and other, available, but American anthropologists and linguists have, for one reason or another, avoided studying the Negro. Only historians and sociologists have given the Negro a fair share of treatment, but conclusions about cultural behavior are difficult to draw from such sources. This is true even for very good sociological studies of the Negro. A study like Myrdal's The American Dilemma is an excellent sociological treatment of the place of the Negro in American life, but cultural information is very difficult to draw from this, especially information about out-of-awareness cultural behavior. Probably much better in this respect is Herskowitz's The Myth of the Negro Past. Here, however, there is a tendency to push everything about American Negro behavior into a mold of a search for African survival, overlooking the obviously important place of cultural innovation among Negroes in the New World.

The American scientific tradition of passing over the Negro probably has its roots in two points of view which are, ironically, quite opposed. In one, what the Negro does is considered merely a sort of reflexion of what the white does, although distorted by mental or temperamental "backwardness." In another, the Negro is regarded as merely a brown-skinned white. The first point of view assumes that behavioral differences between the white and the Negro are innate or inevitable. The second assumes that there are no differences at all. In neither view is there considered to be anything to study. Yet an awareness of cultural differences between American whites of any socio-economic class and many (if not most) Negroes is all-important in diagnosing problems of deviant language behavior. I have never met a speech therapist, educator, or psychologist who was formally aware of the fact that stuttering was a formal device in Negro culture. And studies on age-grading in lower-class Negro culture (all-important for understanding the role of language in the Negro home) are entirely lacking.

If ignoring linguistic and cultural differences in the lower class Negro is gratifying to the social actionist-- who usually has a simplistic notion of what the human family is like-- and is gratifying to the average middle-class Negro who is usually worried about the effect of lower-class Negro behavior on the image of the race, it can only be disastrous to the educator and the researcher whose job it will be to distinguish between different kinds of social behavior on the one hand, and non-social behavior on the other.

For the linguist who approaches the analysis of deviant speech in a Negro child, the problem is usually one of coming to recognize the extent of the differences rather than that differences exist. This, of course, is only partially true. There are linguists, particularly in the American dialect tradition, who hold that ultimately there are no dialect traits among Negroes in the United States which whites don't use, that Negro dialectology is in fact just a sort of mild skewing of white dialect traits which can be traced back to Great Britain. Now this point of view is usually based on American

Dialect Atlas work, which used primarily white informants, not Negro informants, and primarily educated rather than uneducated speakers unless they were old settled people in the region. Since Negroes have tended to move around more than whites for various social and economic reasons, and since the American Dialect Atlas tradition was to find the oldest resident informant, Negroes have been almost automatically excluded from the informant repertoire for any given area, because they will, on the whole, have tended to move in later than some of the white residents.

The second orientation of the American Dialect Atlas which limits its usefulness here is that it has looked only at pronunciation in the sense of particular sounds and particular words-- particularly vowels in particular words-- and at lexical items-- that is lexical items of the type "What do you call this or that on the farm?" It has not looked into the areas where most of the differences do exist, like in syntax and special ethnic vocabulary. If you're looking for the names of things around a farm, you're not liable to turn up special ethnic vocabulary which is used for certain kinds of social relations. Many of the very interesting and important differences in dialect exist in the realm of syntax, in the realm of intonation, and in the realm of certain kinds of pronunciation not turned up by the particular focus of the American Dialect Atlas. If there were grammatical differences they were ignored, since only pronunciation and vocabulary were being looked at; and where dialect switching and other such behaviors were involved they were, of course, totally beyond the orientation of the Dialect Atlas at that time. There was a built in bias in the point of view which doesn't pick up the differences. Therefore it is no surprise that people with the Dialect Atlas orientation could come to the point of view of a commonality of Negro and white dialect behavior, a point of view advocated by McDavid and others for some time.

I might point out that for pedagogical purposes it's a trivial question as to whether some Negroes do or don't talk like whites, or whether all Negroes do talk like some whites. Much is made of the issue for emotional or image reasons, but the main point is how different the dialect is from Standard English, that is from the kind of English you want to teach in the classrooms. And what is also important, and the reason I continually refer to Negro speech, is the socio-linguistic aspect. For many Negroes, moving out of the rural south into urban communities in the North, their total language behavior involves the use of at least some kinds of linguistic behaviors which are different from those used by local whites in the regions where they find themselves. In other words when I talk about Negro speech, I am referring to a sort of socio-linguistic profile of language competence and of verbal repertoire which is in itself different. Even though Negroes involved may use some kinds of dialect behavior which are similar to that used by the local whites, they use other kinds which are not.

I had a very dramatic example of this in Bloomington, Indiana. Bloomington was a stop off place for the Freedom Train where people smuggled out of the South went into the North. Later on, when Chicago became an area for Negro migration out of the South, Negroes stopped off in Bloomington on their way to Chicago. The Negro community in Bloomington today, though small, is one that's been there in many cases for over three generations. There is no rigid housing segregation although the Negroes generally live south of the tracks and are therefore in the lower socio-economic area of the city. There are only two cases I know of in the Negro community where two Negro families live next door to each other. In all the other cases the Negro houses are separated by at least one white family.

Now the Negro children play with white children and go to the same schools, and when you listen to them speak with the white children on the street the dialects are for all practical purposes the same. I did some work in the Negro community in Bloomington and for a while it sounded as if this was a case where Negroes and whites had virtually the same dialect. Then one day when I was taking a group of kids to a lake to swim we started joking ehtnically. Somebody insulted somebody, I started to insult them back and we began making ethnic jokes. All of a sudden there was a very dramatic switch on the part of the kids-- they switched into a quavering voice falsetto, began to use a special dialect grammar, phonology and lexicon. So these children, though they appeared superficially to share the same dialect and did in fact share at least one kind of the same dialect as the local white speakers, had an entirely different ethnically marked dialect which they used for particular purposes. Later when I had a chance to go to some of the local evening dances in Bloomington, I discovered that this kind of dialect was used at the parties.

These children then had a whole dialect range beyond what the whites had-- I'll say beyond in one direction-- because the whites too had a dialect range that the Negro children didn't have. As I worked longer with the community, I realized that this dialect was reinforced from Indianapolis which, in contrast to Bloomington, does have a self-contained, de facto segregated Negro community. Indianapolis is the source of a lot of cultural containment. That's where the rock and roll stations are. That's where the Negro disc jockeys are. That's where a lot of the Negro night clubs are. That's where the dances first come. That's where various kinds of Negro clubs and social activities are, and that's where the clothing styles are first brought in from cities like Chicago and New York and Washington. Negroes in Bloomington quite often go to Indianapolis where they get cultural reinforcement. Then they bring these patterns back although in Bloomington itself they're maintained sort of out of sight and you have to look carefully to find them.

Now what I'm trying to make clear is that when I talk about Negro speech and Negro dialect, I'm referring to a configuration of possible language behavior rather than to a particular dialect with special characteristics. This is not an unusual point of view when one talks about French language. One often includes various kinds of dialect behavior and linguistic usage within what one means by French.

Also some of the dialect differences are incredibly more subtle than one might think. I'm thinking of the anecdotal data where Southern whites in the North have talked over the phone looking for housing and jobs and so forth, and been turned down because people thought they were Negro. This anecdotal evidence has been taken to validate the fact that southern Negro speech and southern white speech are the same. But this goes contrary to the kinds of evidence you get from people who have lived all their lives in southern Negro communities who maintain that the Negroes locally always talk differently than they do. Generally the evidence is that the Negroes can talk like the local whites if they want to but can talk differently as well, can talk in a way that makes them different than whites.

You simply cannot use white reaction in the north to dialect differences as conclusive evidence for dialect differences. Northerners often hear two southern diphthongs, the diphthong in "wouk" and in "I wouk up" and the diphthong in "wauk" as "I wauk around" as the same diphthong. Actually they're quite different to a southern ear. And a northerner is generally not competent to judge this kind of difference. In general a person who is outside of the sphere of a particular linguistic variation is not competent always to judge differences. A good example is that most Americans think that Australians and Cockneys talk the same. The fact is that Australians and Cockneys talk quite differently although historically the two dialects have some relation.

There was some work done in Bloomington where whites and Negroes were recorded and other whites and Negroes were asked to distinguish the voices. The Negroes scored much higher in distinguishing between whites and Negroes than the whites did. Apparently the Negroes are much more sensitive to minor dialect differences that are used for ethnic purposes, probably because they have to be. It's very important for a Negro's social welfare that he's able to distinguish between whites and Negroes, while for whites in the dominant position this is less important. Bernstein says he has observed the same phenomenon among Jews in London and that it is called by many sociologists "sharpening", where a person in a particular group becomes much more acute at distinguishing identifying features of members of his own group.

For the linguist, however, who approaches the analysis of deviant speech in the Negro child, the problem is usually one of coming to recognize the extent of difference rather than that differences exist. The fact that such linguists are usually native speakers of a standard variety of English (with which the non-standard dialect admittedly overlaps on many points) makes it almost inevitable that they will tend to read their own dialect's structure and meaning into apparently familiar constructions in the non-standard dialect. In particular a given non-standard structure will be assumed to have both the structure and the function of the structure it most closely resembles in the standard dialect. If a non-standard structure seems to correspond to two or more standard structures, then the non-standard structure is considered to constitute a "simplification" of the standard. If, however, two or more non-standard structures seem to correspond to only one standard structure, the non-standard structures are likely to be construed as being in free variation and functionally redundant. This is a bias of course in terms of the structure of the standard. Though linguists do this in a sophisticated way, it is the same thing that's done on a very naive level by many school teachers in judging the non-standard speech of children.

Unfortunately this inherent bias which makes it unlikely that linguists will uncover functional or fundamentally different structural differences between a non-standard dialect and their own standard dialect, has received some reinforcement from what for many linguists is a highly authoritative source: Noam Chomsky. Chomsky has suggested that in deep structure, English varies very little from variety to variety, and that most English dialects will differ from each other almost exclusively in relatively low-level phonological and morphological rules. This view, adapted to the standard English bias mentioned earlier, produces a common interpretation, that non-standard speech can be described as a phonologically and morphologically simplified counterpart of standard dialect. I'd like to cite a recent study that's been done to show how this happens.

This is probably one of the best studies on non-standard speech of Negroes that's yet come out, so when I criticize it, bear in mind that I'm thinking of the best, not of the worst. I'm referring to the work done by Labov, Cohen and Clarence Robbins called the "Preliminary Study of the Structure ofEnglish Used by Negro and Puerto Rican Speakers in New York City." In this study they face the problem of the use of what corresponds to the copula in non-standard speech. The whole study is saturated with the view that all the grammatical differences between non-standard Negro speech and standard speech are essentially produced by phonological determinism-- that is that phonological rules have altered the phonological structure of the language in such a way that, for example, verb tense markers have become collapsed or have disappeared, that singular-plural distinctions have collapsed or disappeared. The view is, consequently, that the grammatical structure has been altered at a very low level, although at a deeper structure the grammatical systems of non-standard dialect and standard dialect are retained, and that these deep structure similarities crop up now and then in certain features of the language.

It is a very common view among school teachers for example, that the reason a child who speaks non-standard dialect says "this John book" for "This is John's book" is because he doesn't have the phonology of having sibilants after nasal consonants. Now when one looks around in the dialect, counter evidence rapidly comes up. The child says "This Mr. Jones book" which though it isn't like the standard English "This is Mr. Jones's book" has a sibilant after a nasal. This suggests right away that we ought to look further than phonological pattern to find the reasons for "This John book."

Now Mr. Labov has derived from his own data a paradigm of non-standard dialect with respect to the copula-- and it's quite accurate. This is the kind of paradigm which really reflects the structure of the dialect. "I'm tired, you tired, he tired, she tired, we tired, they tired." This corresponds to the English "I'm tired, you're tired, he's tired, we're tired," and so on. Now Labov points out a certain aspect of the dialect which he's found, and that is that by and large as a historical rule, what in standard English are final "r" and final sibilants, tend to disappear. Now if you look at the standard English, the realization in colloquial English of the copula in these cases you'll see what I mean. He found no case where the nasals completely disappeared. He found cases where final "r" and final "z" had disappeared and so he says: well now, you've still got the copula in this dialect in the structure. All that's happened is you've got a low level phonological rule zeroing "r" and the sibilants in final position. You have the maintenance of the copula because the allomorph is phonologically possible in the first person singular and zero in all the other persons.

Now there are several things that this doesn't explain. It doesn't explain away first of all the alternative explanation and that is that "I'm" is strictly an allomorph of the first person singular pronoum before adjectives and nominals. It is important that one look at this possibility. There are many languages and many varieties of languages where what has happened is that different morphs, that is different grammatical words or words in general have fused together in certain positions because that was the position where they were used together in the original language that the other language or dialects derive from; although linguistically there's no present justification for separating them out. This is an alternative which is quite common in certain kinds of dialects derived from other dialects and Creolized and Pidginized languages derived from other languages. It is so common as a matter of fact that it would have to be dismissed before Labov's kind of argument could be even looked at seriously. But let's pretend that it has been dismissed (although I see no way of dismissing it).

Consider what happens when you introduce a copula because of English teaching in the school. Your first result is to get "is" as the standard copula all the way through, so you get: "I'm is tired, you's tired, he's tired, we's tired, they's tired." You get "I'm is" notice, so you've got a copula introduced into the first person, too. You don't have a double copula in any other forms, you don't get "he is is" or "you is is", though you do get "I'm is." Now why would you get a double copula for the first person singular and no double copula for any of the other forms? Again this would seem to reinforce the argument that there's no copula there at all.

At times the phonological conditioning rules of the Labov approach to non-standard dialect breaks down and can't be used, then other rules are used instead. In some cases they're very vaguely stated morphological environment rules, and where the Labov orientation based on this interpretation of Chomsky really fails is in describing why certain kinds of things are used in the dialect which are clearly different from standard English. He was bothered, for example, by occurrences of "be" as a copula in certain kinds of constructions. And he says, "the most likely hypothesis is that "be" is used for general statements marked by such extra verbal indicators as 'always' 'sometimes' and other markers of the general present indicating an habitual or characteristic situation." He's trying to show that the "be" is predictable in terms of some kind of environment and does not function in any way that makes its function distinct from anything in standard English.

The fact of the matter is that the dialect has very clear distinctions between such constructions as "he tired" and "he be tired," "he walk" and "he be walking." These are very distinct in the dialect. The non-be forms are used for short term action. "he busy", "he sick", "he tired". The be-form is a more long-term, more characteristic or inherent situation, "he be tired", "he be sick", "he be busy", meaning essentially he's busy all the time. The distinction between "he sick" and he be sick" is the distinction between "he's fallen sick today" and "he's chronically ill," in the dialect.

Now this is a very common distinction which turns up in the dialect all over the United States, and this distinction alone is enough to question the Chomsky position about dialect differences. Chomsky may well be right about the majority of dialects-- his position may in fact be a nice way of determining when dialects are close and when they're different, how far down you have to go in your description of the structure to decide how remote dialects are from each other. Chomsky in fact may not have been referring to Negro dialect at all, and he may be the first to say that it doesn't fall into the sphere of the standard kind of English dialect he was talking about. However, what is quite clear is that there is no way that I know of that you can handle the non-standard distinction between "he sick" and "he be sick" and the standard English "he is sick"-- no way you can handle this by superficial rules. Interpreting this distinction requires getting additional information about the real life situation that you are describing, since it apparently is more of a Whorfian type difference than it is a low-order phonological morphological rule. There is much that we have found in non-standard Negro dialect that works like this rather than being handleable by low-level morphological phonological rules. Take for example, the verbal distinction between "he gone" and "he been gone", quite a different kind of structure meaning a very different thing.

In dealing with clear-cut foreign languages or with dialects which are conceptually fairly distinct, theoretical judgements about the relative simplicity of the two linguistic systems or the likelihood that certain of their structures are equivalent can be expected to be cleared up rather early in analysis. In the case of non-standard Negro dialect however, linguistic likes and differences may be much more elusive. In our work in Washington, many of the linguists working with Negro dialect there have been frustrated by the fact that linguistic elicitation techniques which they have traditionally used in their analysis of foreign languages don't work in this kind of situation. For many speakers who are bi-dialectal, their non-standard dialect is in fact a social dialect and is so embedded into the social situation that elicitation techniques, which by the way are formal language focussed situations, produce a kind of formal English which although it may not match standard English is quite different from the kinds of non-standard structures which they would use on the street or in play. For others who are sort of mono-dialectal non-standard speakers, the formal situation of language elicitation produces silence. In neither of these are the kinds of elicitation procedures which linguists normally use, workable. In addition, there are age grading differences that are very important, as I hinted earlier. Children in different age grades speak differently and there is a very noticable dialect shift along with age grade. This hasn't been studied very much, and it needs to be looked into further because of its important implications for the school systems and for language teaching.

Lastly, there is the whole problem of conceptualization of the dialect. Children are not used to thinking in terms of the dialect. They are not used to thinking of the dialect as a linguistic entity. When you talk about language they think of standard English, such of it as they know, and consequently requests to repeat utterances are liable to produce attempts at translation rather than repeats of the original linguistic structure.

What I've done in this paper is to go over a series of problems. I've shown that there are problems in distinguishing between verbal deviation which is culturally conditioned, and verbal deviation which is in fact produced by other factors such as emotional or physiological ones. I've tried to show problems involved in analysis. Once you decide that something is in fact cultural linguistic behavior, then problems of analysis arise in terms of how you decide whether there are differences from and similarities to standard English. And lastly I have tried to show that there are problems in the analysis of the actual linguistic description by native English speaking linguists of non-standard speech.

Dr. Gumperz' commentary on Bill Stewart's paper

Let me talk about some general problems Mr. Stewart has raised. I think his distinction between cultural deviation and individual deviation is a crucial one, and I think it is a partial answer to some of the things we were talking about this morning. It seems to me it's much less important for us to define the bounds of what's linguistic and what's cognitive than it is to find out what kind of problem needs what kind of intervention procedure. If we can define something as cultural, I think that means what we need are some kind of deep techniques, some kind of drill techniques, rote learning of some kind, which is different from the techniques we might use in changing individual behavior. Not being a psychologist, or even a professional expert in language teaching, I don't know just what is needed, but it seems to me the notion is one which we ought to explore and that really needs some experimental work. But from the point of view of the division between culture and individual, I think Mr. Stewart has made a fairly convincing point of the fact that much more than what has been called linguistic by some requires cultural intervention techniques.

The problem of analysis is one which I would be very hesitant to discuss. It does seem to me that too often, with the present social-structure of academia there is a rather deep division between disciplines and a problem of confusion, of cultural lag. Consequently there is a danger of accepting certain purely linguistic notions as given, and then proceeding on these notions in a different discipline. I think we must look at our theories of language not as something static, but as developing. Now I have no way of either proving or disproving anything that Mr. McNeill or Mr. O'Neil has said without doing a deep structure analysis of the material that they have collected. As a matter of fact, I also have no way of proving or disproving their conclusions without going through the same kinds of field work procedures that they did because the kind of material you get also depends on the elicitation procedure that you have used. Similarly, with Mr. Stewart's criticism of Labov's work. There is simply no way of judging these things without going through several days, several weeks, and probably several months of field work and calculations.

It seems to me though that one of the things that we can ask is what kind of a theory of language it is that considers syntax as central. Is this a relevant kind of theory for socio-linguistic analysis? I think it should be questionable rather than assumed that syntax is central and that this native ability, linguistic competence, is somehow located in the syntactic component.

I'd also like to question our notion of language and our notion of dialect. People interested in the theory of language often fail to be specific as to what they consider a language. How do we define the bounds of a natural language or a dialect? Now one way of defining it is by saying that if you need a new set of rules you have two languages. If this is the case then I can cite ample evidence for saying that our present notions of a language don't coincide with such a definition. I know that if I go into any of a number of areas-- India, Southeast Asia, South America-- that what are considered genetically distinct languages have a single grammar.

Another of the important questions that some of my friends who are interested in transformational grammars tell me they are concerned about is our notion of deep structure. Is there a single deep structure for all languages of the world? I don't think so-- I think as we're working we're beginning to find that we need to have different structures for some languages, but we don't know yet what are the bounds of deep structure. Assuming that there is a certain amount of overlap between any set of languages, where exactly do we locate this overlap? In other words, if languages are most different at the level of surface structure, then how far do we have to go up towards the level of deep structure in order to specify the differences? It seems to me that this is a question which we might ask in reference to English, too.

Even though I say I can't judge Mr. Stewart's remarks, I think his example of "he tired" "he be tired" somehow struck me-- offhand, I don't know another way of handling this. I think the question is not whether something that is readily discoverable is a deep structure, but rather where do we locate these various differences? As a person who has worked with bilinguals as well as with dialectical variation, I know that the same kind of social variation, between formality and informality, say, may be located at any of a number of points within the system. In other words in some cases code switching may not be a matter of phonetic switching at all-- it may simply be a switching of morphological codes. One of the most interesting examples is one given by Morris Halle about the differences between English and Pig Latin-- you just add two rules to English and you have Pig Latin. I think that in various kinds of slang-- thieves language, for example-- you get just such differences. Obviously these are surface structure differences, but switching can be at any of a number of levels.

The last point Mr. Stewart made relates to the problem of elicitation. I think this is extremely crucial. I can give you any of a number of my own field experiences. My first field work in linguistics was done in dialects and I was interested in studying tone in a certain dialect of Punjabi. I lived for a while in a village where tone was not significant. Then I went to live in another village where tone was significant and I got some very clear examples of tonal contrasts. A few weeks later I returned to my original village and some of my informants came with me. When I got to the original village I tried to re-elicit some of the tonal contrasts from these informants and was unable to do so. Using the same examples I could not get tone again as a contrast in the context. Now what we need is a theory that will tell us what had changed in our elicitations.

Several people here and in the workshops have pointed out the need for cultural contrastive studies. I think we're not nearly as badly off in this respect as we think we are. We do have the beginnings of a theory, in interaction theory, that will specify some of the variables that we need. I have previously mentioned Goodenough's work on role and status. One of the significant distinctions I think he has introduced is the difference between status and role identity. He says status is a set of rights and duties which are defined as the relationship between occupiers of two distinct social identities. What this work implies is that our notions of status must be abstract. That is, they must have the same kind of relationship to performance that our notions of grammar must have to performance. We must distinguish between X and _Y_ as persons and X and Y as having certain social identities.

We are just at the beginning of this and most of our phonologies and cultural descriptions don't help very much, but certainly the work of Goffman and of Harold Garfinkle at UCLA hear looking into. Certainly one of the first questions that we have to ask is what the culture considers as distinct communicative situations requiring different kinds of behavior-- these are not the same from culture to culture-- and what are the constraints on behavior in particular situations. One of the things that this brings up is that our usual notions of defining populations need revision. We can no longer compare, as Whorf did, the Hopi and the English. English is just not a concept that we can work with in these terms. I think the problem here is the same as the problem in language-- I think possibly we should separate our notion of grammar and language, leave language as an undefined and simply say, a set of grammatical rules which can then be applied to more than one language.

Bailey:

As the convenor of this conference, I want to confess a great deal of distress at this point. While we expected both theoretical and practical papers at this conference, we have somehow not gotten out of the theoretical clouds. I want to press very hard today for us to look at the real issues, the problems in the field. We have invited educators here so they could hear what the people who are working on the throretical side of things are doing and so that we could have some kind of interaction between us. So fat there has been none, because for some unfortunate reason theoreticians are still finding it very difficult to communicate with teachers and others who are engaged in practice. I don't think that any good purpose is going to be served in our trying to pretend that there is not a very serious problem here facing us. One thing that disturbed me yesterday was that we seemed to have taken a backward step from the very hopeful direction which we took at the West Point Conference. In sending the report of that conference to all participants, I had hoped to focus attention on some of the problems raised there. Instead we got into polemics and I want to beg today that we forget about polemics. We have a very important problem. There are millions of children in this country who have, willy nilly to be given some kind of communicative competence. They have to be taught to use the linguistic code which will give them some sense of status, some sense of a worthy self-image, and if the papers we are to get this morning can bring us to this type of treatment-- I shall perhaps be much happier than I am at this moment.

SOME METHODS IN SOCIOLINGUISTIC RESEARCH

Mr. Paul Cohen

Several of you may know that I work with Dr. William Labov at Columbia University on a project investigating the structural and functional conflicts between the English of Negroes and Puerto Ricans and Standard English. It's a study that is financed by the U. S. Office of Education and it is part of Project Literacy. We are attempting to get an accurate picture of the speech of Negroes and Puerto Ricans in New York City, and to determine the systematic divergencies from the speech of the classrooms and from the colloquial speech of New York City whites that may interfere with the acquisition of reading skills. These skills, as has already been mentioned at this conference are not being acquired by a fantastic number of the children growing up in New York City.

As I was looking over the summary of the first working conference here, last October, it dawned on me that it might be a good idea to dwell on some of the ideas, suggestions, and questions brought up at that conference insofar as our work at Columbia bears directly on them. I want to make it clear, by the way, that this is a linguistic project, that we are linguists, and that we are studying language. But we have had to make use of extant socio-logical techniques and occasionally invent new ones in order to study the full range of speech capabilities of our informants and to be able to understand them and their peer group in relation to the community in which they live. There are two main areas of interest alluded to in last year's conference which our methodology might shed some light on and which I want to take up. They can be broadly characterized as follows: First, the social situation and its controlling effects on interviewing and finding out what's going on. Second, the patterns of leadership and prestige of the groups, and their range of repertoire.

We in the project have recently been concentrating our efforts in a small area in Central Harlem which is made up of low income city housing projects and tenements, working almost exclusively with boys between the ages of 9 and 16. The reasons for working with boys are fairly obvious. They're more accessible since normally they're allowed more freedom and their parents are less likely to doubt our motives in trying to contact them. The latter was a problem with girls, so we had to give up the idea of working with them to a comparable extent. Furthermore, since all of us on the staff of the project are male, we have more in common with the boys, so we can make the interviews more interesting both for them and for our own purposes. The age range was selected so that we could get a reasonably good idea of the verbal development and reading ability of the children who, at the same time, had to be old enough so we could have them perform certain tests for us and find out something about their view of society.

The interview forms which we've used, as well as our ideas on what was necessary in the interview situation were in a large part molded by a series of about 50 tape recorded interviews that we did last summer with pre-adolescent boys, 9-12 years old in the summer day camps of the public schools around Harlem. Our intention was to get a large enough corpus of speech in casual, formal and various reading styles-- sentences, word lists, minimal pairs-- to allow us to subject it to phonological, syntactic and content analysis. It soon became apparent that it was relatively easy to get formal style, of course, and also reading style when the child could read at somewhere about the second or third grade level (a task which many-- even 12 year olds-- could not perform) but there was very little casual speech. And the need for casual speech is very acute, as has been pointed out be Dr. Labov in his 1964 Columbia University dissertation. "The Social Stratification of English in New York City." It is apparently the most systematic form of speech; and there is no way of knowing, if you don't get some kind of casual, informal or spontaneous speech, whether what you're dealing with, especially syntactically, is at rock bottom of what's really going on, or is an admixture of a few partially assimilated rules with the basic speech patterns.

We realized there were several causes for this lack of casual speech. For one thing you could almost hear a pin drop while the kid was talking, in what was supposed to be a truly informal interview. We were going around to school playgrounds, and we'd try to get a quiet room from whoever was in charge of the playground and the two of us would sit there. But it was a classroom. I'd try to sit on the desk and smoke and slouch or do something-- but it didn't make much difference. As soon as we got into the school room all you could hear was, "Yes" (softly), "No," "I don't know," and unless you gave them a command to do something like recite a rhyme, that would often be just about the only response. Then of course there was the fact that we were adults-- strange adults at that-- and that the relationship was unfathomable to them-- neither parents nor, as we kept telling them, teachers. Furthermore, as most of the interviews in this series were done by myself and one other white interviewer, there was also the problem of a white interviewer interviewing Negro kids. It was an unnatural situation for casual speech in any case, because we were unknown to the children. The interview was supposed to try to force casual speech because it's on casual speech topics, but you don't talk casually to a stranger (unless you're at a meeting like this where everyone's a stranger). Then, too, we had the tape recorder present. Nothing could be done about that, naturally, if we were going to have any kind of permanent record of what was going on for future analysis, but most of the kids knew what a tape recorder was, and a good many of them didn't want to be put on the record. Many of them had suspicions about the interview anyway, no matter what was told them. Whether we told them the truth or a lie didn't matter since we just wanted to hear them talk, but from their standpoint we could have been almost anything-- welfare investigators for example.

Now at the same time as the other interviewer and I were doing these interviews throughout Central Harlem, our co-worker Mr. Clarence Robins was more or less concentrating his attention on a summer day camp and a night-time center for teenagers and pre-adolescents, trying in some way to get hold of one or two kids who knew each other, by sampling randomly. Through the peer group sections of the interview: questions about "who do you hang out with?" "Do you belong to a club?" "Does it have a name?", things like that-- he was trying to get some kind of match so that we could get a group together. In other words, we got child No. 1 and he mentioned his friends were No.s 2, 3, 4, and 5. We got hold of 2 and 3 and found out who their friends were, and in this way we tried to trace the social networks. Finally we got a group together, six the first time. At least four of the six were core members of the group and the other two were peripheral members. We told them we wanted to take them on an outing, a cook-out, in New Jersey. We got parents' permission slips and so forth and we took them out. As you might expect, in their own group there was a great deal of spontaneous and casual speech, especially out in the open. We tape-recorded the session in the car and outside, and the sound was very good, even in the car, but then we found that we couldn't tell who was talking at any time because several kids were usually talking at once. The kids themselves could not identify *their own voices; and when we ourselves could identify voices*, we usually couldn't get the whole sentence because someone was talking at the tag end. Or vice versa; we'd get the whole sentence, but we couldn't possibly figure out who said it because three of them said it at once with slight variations. But there was a preponderance of casual speech in the interview.

We decided then that the only way out was to record them as a group but separately. So we got them together at our office for a party-- soda, potato chips, ball-playing in the unoccupied room next door, anything-- and then we sat them down around the table. By this time we'd gotten to know them somewhat so the situation was less strained. We hooked each one of them up with a separate lavalier microphone and a separate tape recorder and we also had a tape recorder and a table microphone to record the whole mass of incomprehensible sound at one time. The interview was successful in every way you could ask for, and although in the recording of the group as a whole very little of the total volume of speech could be understood, when each separate tape was played, there was almost never any doubt as to who was saying what. As a matter of fact, a great deal of speech could be heard on these individual tapes that was not heard by anyone during the interview or on the group tape. We even managed to isolate a new type of speech, what Dr. Labov has called "private speech in public." Apparently the kids thought the microphone wouldn't pick them up if they whispered softly enough, and of course they also heard all the noise around them so they felt sure that no one could hear them talking above the noise in the room. But the microphone did pick them up, and they were saying the most incredible things about each other while looking at each other-- things that they could never possibly say otherwise.

This interview helped us to learn a great many things about relationships in this group. The group was called the Thunderbirds. I think they have since changed their name to the Lions-- names change at the drop of a hat or less-- but at the time it was the Thunderbirds. We had five of them there. Now this is a very strange group, a group unlike any I've ever heard of. It has two leaders who are co-equal, absolutely co-equal in their own spheres. The two leaders were named Boo and Roger. You would have to call Boo the verbal leader. He's always talking; he's always interrupting everybody. He excels at "sounding" ritual insults. When it comes to talking, Boo's in charge. If we ask the group a question to try to get some kind of narrative, some personal experience out of them, and somebody tries to say something, Boo will shoot right in, cut him off, and finish it up. Roger is the doer of the group. I wasn't on the first group interview when they went out to New Jersey, but apparently Roger decided he was going to go fishing, even though he didn't have any fishing gear. So he managed to scrounge up some fishing tackle, borrowed a hook, dug up a worm, and was fishing. Roger is the kind of kid who can just accomplish things, but he doesn't talk very much. Most of the time Boo will shut up if Roger wants to say something, but if anybody else tries to talk, Boo is in there stepping on him.

Boo and Roger are both 12. Now Ricky is 11 and in some ways is the head of the Junior or Baby Thunderbirds. He doesn't really know which group he belongs to. He's on the lower level of the upper age group and the upper level of the lower age group. Money is a cousin of Boo's. George is his real name, but Money is the only name he's known by on the streets. He's ten years old, and the only way to describe him is as Boo's yes-man, a fact which you could never find out in a single interview, or in most group interviews. David is a peripheral member of the group who happened to be around when we were collecting guys. David got dragged along, but he can take care of himself and is sort of two-thirds of the way into the group.

We learned a great deal about the group just by watching them, even before we started talking. One thing we learned was that we couldn't trust the reports of what was going on from the people who ran the center, even though these kids were at the center all the time. For instance, Clarence spent about three weeks tracking down Ricky because he was known as a big trouble-maker and the leader of the group and had been barred from the center for being a disruptive influence. It turned out, in fact, that he was a very low second lieutenant; so it seems that the people at the center, although they work with them for months at a time, don't really know what's going on in the groups.

But as soon as we got them together, you could tell what was going on when they chose seats. Anybody could have sat anywhere at the table. Boo sat down at the head of the table-- immediately. Roger sat down in a corner and pulled his chair away from the table a little. Money pushed his chair right over next to Boo's. David sat at the end opposite Roger, also somewhat away from the table, and Ricky was at the last corner (that is the one that was left) since we had five seats open. When the talk began, Boo controlled the group verbally, by motions of his eyes, with a punch in the ribs or with anything he could pick up and throw. Everybody talked quite a bit when they got a chance (Roger told Boo to shut up, so Ricky got a chance to talk)-- everybody except Money. About the only verbal communication between Boo and Money, who was almost hanging on Boo's elbow, was "Shut up, Money," as soon as Money started to say something. This was very strange because we had previously given Money an individual interview and found him to be quite verbal. We have a section in the interview where kids can exhibit some of the best insults they know, and Money did quite well. When we got around to the same kind of technique during the group interview, he couldn't say a thing when Boo started talking-- Boo had absolute control over him verbally. David is quite a verbal guy if he gets a chance to talk, and he usually does because he doesn't stand for Boo's nonsense. But a great many of David's sentences would be ungrammatical by classroom standards, partly it would appear because they were usually more complex than the sentences of the other kids, so that he had more chance to be ungrammatical.

Before we analyzed their individual interviews, we expected that the verbal leaders of the group would be the best at reading. We assumed that speaking and reading abilities correlated-- I think that's a reasonable working assumption. It happens that with this group almost the exact opposite was true. Of course, David was a very good reader, something that might be expected since he was speaking in complex sentences, but he made a great many grammatical errors which led him to misinterpret many passages. (Our phonological analyses of his speech and the data from some rudimentary audio_ perception tests given him have led us to believe that this is intimately related to his reduction of final consonant clusters in speech. This fact has made such morphemes as the -ed past-tense marker virtually devoid of information for him, even in reading. His is a characteristic case.) But he could get the general picture of what was going on in a sentence and he could read some of the tougher words. Boo, the verbal leader, couldn't read, in any meaningful use of the term.

We have a set of readings, the first page of which is used for determining if the kid can read at all without embarrassing him. We hand him a card, and the instructions with the card are "Read these numbers from one to ten." And the numbers from one to ten are written out. Now if he's thinking at all, he'll say, "One, two, three, four, five, six, seven, eight, nine, ten." The second card is given to him with the instruction, "Read the days of the week." In all probability he'll assume it starts with Sunday or Monday, but the card in fact starts with Tuesday and goes around to Monday. So if he can't read, if he doesn't get Tuesday right or even try to pronounce it, we don't even bother with the rest of the reading. This way we don't embarrass the kid and we still find out if he can read. Then, if he can read, we go on to sentences and word lists and the like.

Boo read the numbers, and the days of the week, and then he read three words of the first sentence and said "I can't read this." Roger on the other hand, who never opened his mouth, was a very good reader, and so was Ricky. We've gotten similar results with other groups-- the lack of an ascertainable relationship between a kid who's really verbal and a kid who reads. Now I don't know what the mechanism is-- all I can do is present you with these facts. It's sort of horrifying because it seems so anti-theoretical. But it's the truth.

As important as this interview and subsequent group interviews with other clubs were, we realized that we were introducing a very strong bias into the sample of the groups that we were getting, trying to look into these social networks from the day camp centers. The groups we were most interested in, the delinquent teen-age groups, were exactly the groups we weren't meeting. These kids would not hang out in the center because they consider it too "square." They'd never be found dead in day camp after the age of 12. Of course you can see these kids all the time if you walk down 113th Street near Lenox Avenue and look for the nearest lamppost or the nearest doorway or the nearest candy store, but to get to know them is quite a job.

About two months earlier we had hired an interviewer, a lower-class Negro who was an acquaintance of Clarence Robins'. He did, and is still doing, a tremendous job for us, getting these people and interviewing them, especially teen-age kids. He doesn't look much older than 18 himself, and he fits into the groups very well. His name is John Lewis, and though he's not truly an indigenous interviewer since he grew up in Trenton, New Jersey, he has lived in Harlem for several years and was at that time living pretty close to this area, so he knew it well.

Clarence Robins first made the contact with one or two of the boys in a couple of these teen-age gangs and KC (John Lewis) followed it up from there, trying to round up a group and getting them together for a group interview (at our place again). By this time we'd purchased a video-tape recorder and we were showing them tapes of boxing matches. Our techniques for opening them up were getting more and more polished. Naturally all the kids that we got together in group interviews had to be interviewed singly. We usually tried to get two or three before and the rest of them afterwards. Sometimes we'd have to give them a quarter or buy them a soda or something to get them to sit through the taping, perception tests, and correction tests and all sorts of other tests, and just to talk.

We found that in order to get any kind of relaxed atmosphere, any casual, informal, lively speech from these kids, we had to revise our teen-age interview. We had to make it more "hip." What we did was to sit down and translate the questions from standard English into "hip" English. KC had to know what was going on or he would never have been accepted. So that if the introduction to the interview in a normal, casual style was something like: "We'd like to find out the things you guys do around here and where you hang out and a lot of things about the games you used to play when you were a kid," we might say on the hip interview something like: "We'd like to dig what's happnin' roun heah and see what makes you dudes tick," or something of the sort. It was very effective.

We rented a room out in the middle of the Central Harlem area where KC could bring the kids up and interview them at any time. In the past we had always tried to get hold of a kid and put our meat hooks into him and never let him get away, because most of the time we'd never see him again. We couldn't let a piece of the interview go or say "Well, next week we'll pick him up-- we just invented this new test-- we'll give it to him and see what happens." That was impossible. But with KC out in the streets all the time--a permanent field worker, really--reporting in every few days, halfway living out of this room and taking the kids up there, we found we could do almost anything we wanted. We could always contact them again. All KC had to do was look out the window and yell out, "Hey, Joe, come on up."

As soon as they realized that we weren't trying to get anything from them, everything was fine. But KC got into a very ticklish situation-- he was contacting two groups at once, and it happened that these two groups were not on the friendliest of terms-- in fact everytime they saw one another they tried to get six against one and knock one of the other guys off. So KC was placed in a position in the middle that was hard for him to extricate himself from. He did manage to stay neutral, and he even called the leaders of the two gangs with their warlords and other paraphernalia up to the room and made them sign a peace treaty-- at least for the time that we were going to be there. We asked them at separate times what they thought about the peace treaty-- was it going to work? And each one said, "We'll keep to it, but of course the other guys won't." So far both have; so far it's been effective.

One of these groups was called the Cobras, at least for that three days we first got hold of them. These were delinquent kids. They were stealing, drinking, smoking, robbing purses, and the like. We managed to make friends with them-- we interviewed all of them singly, had them in as a group at the office and took them out on trips-- the same general routine. About two weeks later we found out that the group was breaking up. So we had four of them over for a group interview. Four or five is about optimum size if you're going to keep a little control over what is going on. You can't really control-- it's virtually impossible to keep to the interview form if you want casual speech. You've got to let them roam. You can occasionally stick in a remark and try to direct the topic around to card games if you're interested in finding out what's going on with cards, or to the peer group, or to what do they think happens to them after they die, or something of this sort. But it's very difficult.

Now the leader of the group was taking off by himself, his right-hand man was taking off by himself, and two of the other boys were apparently joining another group called the Bohemian Brothers. These Bohemian Brothers were a new group politically way out on the right somewhere. They've been called five percenters by some, they're some kind of splinter group of the Nationalists or Muslims. They look up to Malcolm X but they're followers of the Prophet, a fellow who's now in Matteawan, the State Hospital for the Criminally Insane. This is the kind of prestige model that these kids have. This fellow sends down pronouncements, and for everybody in Harlem who follows this sect, that's what's going to happen, period. Since he's some kind of Black Muslim, eating pork is out (the most reprehensible thing you can say about somebody now if you're in this area and you're black and he's black is that he's a "stoned pork chop," that he eats pork, implying his "squareness and so forth, but this fellow at Matteawan has carried things a little bit further. He's decided (or at least has been interpreted as having decided) that all meat is out, "because you don't put dead meat into live meat" for some mystic reason that I can't explain. Milk, eggs, cheese, everything like that is out because they're meat products.

So when we had these kids over, we had KC slicing sandwiches. We figured with lettuce and tomato on whole wheat bread we couldn't go too wrong, but KC made the mistake of putting mayonnaise on the sandwiches and one of the kids was smart enough to realize that mayonnaise comes from eggs and we had about a 10-minute argument with them that they were either going to starve or eat the mayonnaise. Apparently eggs and milk are OK, but you just stay away from them as much as you can.

Now the two guys who joined this group were the two most verbal guys of the four-- they had talked to us quite freely and one of them I personally think is extremely intelligent. When we asked them why they joined they said they "wanted some action"-- there wasn't enough action in the old group. Well, we had these two kids in several interviews-- once when they were Cobras and then when they were members of the Bohemian Brothers. They had absolutely changed within the space of three weeks. We had one huge interview-- 13 or 14 guys and four interviewers: Clarence, myself, Dr. Labov and KC, and they wouldn't talk to us. If either Dr. Labov or I tried to say anything they just clammed up. They'd say something mystical to the other guy, "It's too deep for them" or something like that. I asked one kid his name and he looked at me like I was a spot on the floor, so finally we just had to depart and leave the interview to KC and Clarence.

The interview was successful in one sense, in that we found out what was going on with these kids, but other than ritualized misstatements of the Koran we never got much casual speech. But we did find out how much control the group exercises over each of the kids, and the control is fantastic. The leader whose "attribute," his adopted Arabic name, was Quahab, only had to dart his eyes or something and that was the end of it; everything stopped. And, as with all factions of the Black Nationalist movement, this group is growing rapidly.

Implications of this kind of hostility as far as teaching is concerned, need hardly be mentioned. We feel that what is clear from all of this is that if we want to do linguistic work, work with the kids' language, we must study their behavior as a whole in order to study the full range of their linguistic behavior. To do this we must strike a balance between naturalism in the situation and some kind of control of the situation. We've decided to lean, if necessary, toward naturalism in the situation hoping that we get some kind of superordinate control in terms of the fact that everybody was there in the same situation even if we didn't ask them quite the same questions in the same order. In addition, it is apparent that if successful attempts are to be made to teach children anything new-- including reading-- we should ascertain in advance any beliefs and attitudes which are contrary to our purpose. We must combine knowledge from linguistics, sociology, pedagogy and other fields in order to develop effective techniques. We can't afford to overlook any avenues, whether theoretical or practical, to success.

A CURRICULUM DEMONSTRATION PROJECT FOR TEACHING LITERACY SKILLS TO DISADVANTAGED 7th AND 8th GRADERS

S. Alan Cohen

INTRODUCTION

We have a fire raging within and about the slum schools, and the feeling I get is that most school men are trying to form bucket brigades with porous thimbles to try to put it out. Meanwhile many behavioral scientists are busy inventing more efficient and exciting water pumps for future fires, and not too many people are left to man the pumps that presently exist. What I'm going to describe to you is an attempt over the past five years to put out the fire with old but still unused pumps. We asked and answered the following questions: Number 1. What principles of learning, what principles of psycho-social development, what principles of pedagogy do we trust? And second, can we translate these principles, drawn from basic research into action? In this latter area I think we tend to underestimate the "young science of psychology." Its youth is always used as an excuse-- "It's a young science and therefore we shouldn't expect what we do in the learning lab to be applied." I don't think that's so, and I want to describe to you a project which attempted to apply them.

HISTORY OF SKILLS CENTERS

The answer to both of these questions-- what principles can we trust, and can we translate these principles into action-- led to a technique that I call Skills Centers, a technique for upgrading the reading skills of disadvantaged children. In the past four years this technique has been tried at a Massachusetts reform school for early adolescent boys, in some demonstration classes at Boston University, in the Mobilization for Youth Summer Reading and Homework Helper Programs, in some Job Corps camps, and in other projects. This year it was implemented at Westinghouse Vocational High School in Brooklyn, and the skills center approach will be demonstrated this summer at Yeshiva University's NDEA Institute for Teaching Socially Disadvantaged Children.

DESCRIPTION OF POPULATION

Let me specifically describe a version of Skills Centers which we conducted last year in a New Jersey City of about 55 thousand people. The school covers grades 7-9 in a city that once boasted the finest shopping center west of Manhattan. Now stores lie empty, factories lie dormant, political skulduggery abounds. The school superintendent lacks the qualifications established by the school committee that hired him; he is the mayor's friend. School positions are bought with money and sex. Staff morale is low. The 85-year old school in which we worked houses 600 pupils, 150 more than the building design specifications called for.

Slightly more than half the pupils are Caucasion-- children of semi-skilled and unskilled laborers from eastern European backgrounds. About half of these children come from bilingual homes. About 9% of all the pupils are non-English speaking. About 31% of the pupils are Negro and roughly 20% Puerto Rican, so about half the school is Negro and/or Puerto Rican and about half are Caucasian children. Over 50% of the children come from families with average incomes below $3500 a year. About 30% of them are known to local police and youth board for delinquency. Average test scores for the school on the Iowa Test of Basic Skills show: Reading, 10th percentile; Vocabulary, 8th percentile; Language Usage; 15th percentile; Study Skills, 15th percentile; Arithmetic, 11th percentile. Scores have been steadily declining over the past five years correlate to an influx of a lower socio-economic group into the school district.

In this school it cost us about $1500 to implement Skills Centers, and the money west mostly for non-expendable materials. An SRA Reading Lab, for example, can be used next year and the year after until somebody burns it. The funds were provided by the Ford Foundation.

SEVEN PRINCIPLES OF LEARNING

Now remember the question we asked was: What principles of learning, psychosocial development and pedagogy do we trust? In the last chapter of Hilgard's Theories of Learning he lists 15 principles of learning which he says most learning theorists would agree upon.

I went through the 15 and selected seven. These are the seven principles of learning that I think we can trust, that everybody talks about but too few in the schools seem to translate into action. Some of them seem almost trite because they have been around so long. First: Capacity determines when to teach what. We know of course that the schools don't operate this way. The schools predetermine when and what and then the pupils come in and are fitted to it. The second principle, that motivated kids learn more readily then unmotivated, really deals with some subprinciples of motivation. One of the things that the school people usually overlook is the problem of controlling intensity of motivation. They overuse extrinsic reward rather than intrinsic reward as a motivator. They overuse negative rather than positive reward as a motivator. Although both positive and negative rewards may lead, equally to certain types of target behaviors in learning, Skills Centers uses positive reinforcement because we think it will cut down on some negative psycho-social byproducts. And as a last sub-principle of motivation there is the notion that continuous success builds in frustration tolerance.

The third principle of learning is that the self-selection of realistic goals tends to bring about a more effecient learning curve. The fourth principle involves active vs. passive learning-- When the individual is fully involved, even motorically, in the thing he is doing, rather than being just the recipient of a limited group of stimuli, he is actively learning. The fifth principle is that tasks that are meaningful to the learner tend to bring about more efficient learning. This involves understanding the why of what he's doing at any given time, not just a general awareness that education will help him get a job in five years. The sixth principle is that the timing of feedback and the scheduling of feedback to the learner's responses are crucial to the efficiency of learning. And the seventh principle-- the one we can't do much about-- is that the past history of the learner certainly has some influence on how he's going to learn.

SIX PRINCIPLES OF PSYCHOSOCIAL DEVELOPMENT

The principles of psycho-social development underlying Skills Centers are, first, development of social responsibility, the feeling of responsibility toward others in the immediate environment. We built into the Skills Centers methodology, for example, team learning where small teams work together to reach a goal. The method is so structured that the kids have to depend upon each other to get to a certain terminal behavior. The second principle is the assuming of personal responsibility, enhanced by a classroom based on self-directed learning, in which the individual is self-directing both in the actual learning situation and in his own evaluation. The third principle is the promoting of social-personal development not just by individualizing instruction, but by personalizing the methods and materials used in the program. The fourth principle is to stress positive rather than negative types of rewards in the hope of reducing negative behaviors. The fifth principle involves building in positive reward and continuous success, a situation which some people think builds tolerance for frustration. Finally, the sixth principle is the _opportunity to control one's own destiny_. We have limited freedom in the classroom, as in life, but still within those limitations it seems to me tremendously important to give a person the feeling that he does have some control over his destiny.

PRINCIPLES OF HIGH-INTENSITY LEARNING

Now these are the learning principles and the principles of psycho-social development that underlie the method. I also asked, what pedagogy can we trust? And I have a very simple model of what an ideal learning situation would be in the formal school. It would be a pedagogy which individualizes content, level and rate. That is, the skill I teach a youngster at any given time would be a skill that he needs to learn. If a teacher stands up in front of 30 educationally disadvantaged kids and teaches some skill that ten of the kids don't need, the excuse usually is that it won't hurt them. The fact of the matter is that it will hurt them, because these kids have _such_ deprivations, they have so many needs, that it will hurt them in the long run to waste the limited amount of time we have to compensate for their deprivations. The content of instruction must match the child's needs. Then, whatever content or skill we're going to present would be at the level that a youngster can handle. Often kids learning what appears to be the same thing are really learning it at different levels. A personalized program would allow for this. And finally the rate at which we present the skill or content would vary from individual to individual.

Now all of this would take place in what I call a high intensity learning situation. That is, the stimulus response situation would be intensified so that no kid is waiting his turn. A high intensity learning situation is a regular series of personalized stimuli-- that is personalized in terms of content, level and rate-- being presented to the individual, and the individual is continuously responding. He's not waiting his turn. He doesn't have to say, "Well, maybe I'm next," either in fear or in hope. These are the pedagogical principles behind the Skills Centers.

RESEARCH DESIGN

Preparations for the program in the particular school I'm going to describe were started during the fall and early winter of the academic year. We planned to implement the program in January. This left a minimum of time for testing the program since the experiment would be limited to less than six months. Considering the length of time needed by the teachers and the pupils to adjust to the new routine of the self-directing classroom, and considering the 15 days we needed for post-testing, *the experimental treatment was in effect from February to late May.

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* We snuck into this "action research project" a "basic research project" in visual perceptual development which required extensive testing.

During the preparation period three teachers formed a teaching team and met daily to modify existing materials and to order new materials that could be used in a self-directing classroom. In addition a battery of pre-tests on the Iowa Test of Basic Skills and the Basic Test of Reading Comprehension (BTRC), were administered to all seventh and eighth graders in the school. The BTRC is a reading survey designed for socially disadvantaged children and youth by this author and Dr. Robert Cloward (Assistant Director of Research, Mobilization for Youth). It yields a rough estimate of reading level in only six minutes of testing and is standardized on a socially disadvantaged population. Coefficients of validity for the experimental edition with established standardized reading tests have ranged from .72 to .81.

The project used three adjoining rooms, two of which were connected by a converted cloak room (7 by 23 feet). Each 23' by 25' classroom was designated a Skills Center-- one for Study Skills, one for Comprehension Skills and one for Vocabulary Word Attack Skills. Each teacher became a specialist in one of the three areas and was designated a director of a Skills Center, with responsibilities only in the materials and methods for teaching one of the three skills areas. The Comprehension teacher functioned as diagnostician-- using asalysis of error patterns of SRA Reading Labs as the basic diagnostic technique. This teacher's job was to alert his team to specific problems of each child. The teaching team met together during the first and last period of the day for planning, diagnosis and materials development. Actually the teachers would develop ideas and models, and they used a group of volunteers to develop the "teacher-made" materials.

The research design called for a delimited range of intellectual potential as measured by intelligence tests. The range included the statistically average population as well as low and high average, but excluded the severely retarded and the highly intellectually gifted. Intelligence test norms with socially disadvantaged children usually yield a modal range of 80 to 100 IQ. The population was delimited by eliminating those pupils who had no oral-aural English language skills (Spanish and Polish speaking), as well as the top 64 achievers in grades seven and eight. Retarded and brain damanged children had been eliminated by the school system via isolation in a special program entitled "Opportunity Classes." This left approximately 360 seventh and eighth graders from whom experimentals and controls could be drawn for the experiment.

The 360 seventh and eighth graders were ranked from poorest to best readers on the basis of the BTRC regardless of actual grade placement, producing a ranking of ungraded seventh and eighth graders ranked according to reading achievement. Every other child was then assigned to Controls, so that what we had were 180 experimentals and 180 controls roughly equated on all important variables. The lowest 20 readers in the Experimental group were designated Experimental III.

The remaining 160 were further subdivided into two equated groups, one of 60 (E2) and one of 100 (E1). This technique plus attrition resulted in the following N's for the experimental treatments:

E1 ---- N = 87

E2 ---- N = 56

E3 ---- N = 18

E1. Consisting of 87 pupils, this group was subdivided into three classes of low, medium and high in school achievement based on standardized tests, grades, and teachers' and guidance director's evaluations. The regular three class hour block of literature, language arts and social studies was replaced by Skills Centers. Each of the three groups would begin the first period in a different Skills Center, shifting to another Center at the end of the period, and to a third Center at the end of the next period. Thus each class received one class hour (44 minutes) in each of the three Skills Centers under each of the three teachers daily.

E2. Consisting of approximately 56 pupils this group was divided into higher and lower achievers according to the same criteria as E1. E2 received two class hours daily of Skills Centers program which replaced the literature and language arts courses. Unlike E1, E2 attended regular social studies classes. The Vocabulary and Word Attack Skills Center was included in the Comprehension and Study Skills Centers, but with less intensity than experienced by E1. Thus E2 was in general a less intense version of E1 with children coming in contact with only two of the three teachers.

E3. These 18 pupils were severely retarded readers, many of whom had language usage problems. They received two hours daily of Dr. Myron Woolman's Accelerated Progressive Choice Reading Program (now published by SRA as Reading in High Gear), supplemented by other simple word and auditory discrimination exercises, tachistoscopic training and simple readings. The treatment was a pilot study to allow the teacher to evaluate Woolman's program as a method of teaching hard core underachievers.

Controls attended regular classes in language arts, literature, and social studies. Classes were not ungraded as in the experimental program, and each class represented a homogeneous group according to criteria established by the guidance department of the school. The criteria included grade point average, achievement scores, and subjective evaluation. Instruction in general was traditional, consisting mainly of lecture, discussion and homework. To balance for Hawthorne effect, some Control classes were provided with SRA Reading Labs.

DESCRIPTION OF SKILLS CENTERS OPERATION

Now let me describe to you just how these centers operated. The kids' reading skills were pre-diagnosed on the Iowa and on the Diagnostic Test of Word Attack Skills. We then put up a huge chart in one of the rooms; the kids' names were listed horizontally, while vertical columns listed the skill areas we were teaching. The areas we chose to teach were to some extent determined by the materials we could get. It seemed pointless to pretend to deal with skills that we didn't have materials or method to affect. Then we put an X under each skill that a kid had a weakness in. On the basis of individual needs, the teacher wrote out a personal schedule for each kid.

A typical 44-minute class schedule for one child in the Study Skills Center might include 15 minutes on English 2200, 20 minutes on Programmed Geography and 5-10 minutes on Gates and Peardon Practice Exercises. Pre- and post check tests were given at the kid's own request. Each kid had a schedule; the groups as a whole moved from Center to Center at the end of each class hour. However, if a kid got hooked on something and really wanted to stay with it, he was allowed to do so even when his group moved to the next center.

To achieve self-direction we used as much programmed instruction as we could find. Other materials were restructured with answer pages or answers were posted on the ANSWER CORNER bulletin board. This allowed youngsters to check themselves immediately. Everything they did they measured in some way; the time, the number right, the number wrong. On speed reading exercises, youngsters computed words per minute converting to and from words per second, and in the process learned long division. Within two weeks every youngster in the lowest group could do long division, something their math teachers couldn't teach them in eight years of school. Scores on everything were recorded by the pupils themselves on progress charts kept in folders stored in the center of the room. The charts were in the form of graphs. These children had scored very low on graph and map reading on the Iowa, but we never formally taught graphs. Instead we had the kids plot their reading skills progress on bar graphs and histograms sometimes with three or four items on a single graph. All the youngsters learned to read and manipulate graphs because it was meaningful to them.

THE ROLE OF TEACHERS IN SKILLS CENTERS

It took the youngsters about two weeks to get used to self-direction, but the teachers were more of a problem. About a month into the program they were terribly uncomfortable about not "teaching." Actually the teachers were doing more than they thought. In order of priority they were first arranging what I call a therapeutic classroom atmosphere. The classroom must be a comfortable place where teacher evaluation as a judgement is eliminated, or at least reduced. Then the teachers were teaching kids how to operate in the situation--how to use the materials, how to measure themselves-- teaching them how to learn. Next they were matching materials to kids; each Monday each class was regrouped into subgroups according to diagnosed levels and needs. We got "natural" subgroups, that is kids who had the same needs worked together on the materials. We never bought more than five copies of any materials because we wanted to discourage teachers from teaching all 30 youngsters the same thing at the same time. Do you remember our classroom chart of diagnosed skills weaknesses? When a kid suddenly achieved in a certain skill area, up went a red mark or a star on that chart which meant he had finished-- he had achieved in that skill and he could move on to another skill area. The Comprehension Skills Center specialist as diagnostician told the other two teachers where each kid's problems were and then appropriate materials and instructions for using them were provided for each youngster.

Since the teacher was not lecturing, but was instead floating around the Center, he was available to give first aid on a one-to-one basis. The minute a kid had a problem, the teacher was right there. Since we never had enough materials for the problems that developed the teachers were continuously developing ideas and models for new materials.

We had five hypotheses about this method:

H1 Experimentals 1 would exceed all other groups in achievement on the ITBS subtests of Vocabulary Reading, Language Usage and Work Study Skills.

H2 Experimentals 1 and 2 would exceed Control 1 in achievement on ITBS subtests.

H3 Both high and low achievers would benefit equally from Experimental Treatments 1 and 2.

H4 Pupils would show an increase in motivation for school work and a reduction in negative acting out behavior as measured by teacher evaluation and unannounced spot checks by observers.

H5 Non readers would attain a level of functional literacy as a result of the intensive Woolman Program.

FINDINGS AND CONCLUSION

Gross measurements of total means between experimentals and controls showed no statistically significant differences. It appears that the duration of the experimental treatment was far too short to affect measurable change on standardized tests. Thus H1 and H2 cannot be accepted.

On comprehension E1 and E2 scored .4 year above controls. Controls scored about .3 of a year above E1 and E2 on vocabulary. Differences on other reading skills among E1 and E2 and Controls were negligible. We found no statistical differences between E1 and E2. H3 was rejected-- high achievers gained more than low achievers (to the .05 level).

H4 was accepted. The high intensity learning technique was successful with severely retarded socially and culturally disadvantaged seventh and eighth graders.

H5 was rejected. The Woolman program (E3) was not notably successful in solving the literacy problems of the severely retarded readers. The teacher was forced to resort to many teacher-made materials. The program is usable with modifications, but it is far from satisfactory. This means that an adequate reading program for illiterate socially disadvantaged adolescents is still unavailable to professionals in the field.

Despite the lack of measurable results, one thing is clear from our Skills Centers experiences. Culturally deprived low achievers can become self-directed learners if we teach them how. Individualized instruction can be achieved with a 30-1 pupil-teacher ratio if schools are willing to innovate. Furthermore, acting out delinquents, low achievers, and high achievers all appear to learn well in the Skills Center program. Learning increases and behavior problems decrease. In every experiment that used a variation of Skills Centers with all types of pupil populations, teachers reported a reduction of classroom discipline problems.

I would like to conclude with one anecdote, which I think illustrates the effect of Skills Center as well as anything else I might say. This incident happened during a summer reading program using Skills Centers at Mobilization for Youth. The funding organization dropped in unexpectedly one morning and wanted to observe the Skills Center. Unannounced we walked into the first room, the Vocabulary-Word Attack Skills Center, and found one kid asleep in the back of the room, not bothering a soul. The others were going at it--working away at various word games, taped programs, programmed books and kits. One of the kids came over and I asked him where the teacher was. "Oh, he's not here-- he's sick."

"Why didn't somebody notify the office?" I asked.

"Youdonhafta notify the office," he said, "we're operating." This is what we have seen going on in our Skills Centers this is what I call high-intensity learning.

Dr. Smiley's comment on S. Alan Cohen's paper

Let me make one observation which is essentially a reaction to this paper, and then perhaps go back over some of the things that I have been thinking yesterday and today, and some of the implications that occurred to me as a practitioner.

The thing that I note especially in the program that Dr. Cohen describes is that it not only does not exploit the social situation in which I am inclined to conceive of language and language learning taking place, but it appears in effect to avoid the kind of interaction that seems to me to be an irreducible requirement for language development. Now I would like to see what is actually happening in his program. I have seen skills programs of this kind in operation in schools and in women's Job Corps Centers, and in those situations, the approach did not seem to me to be as effective or as productive as Dr. Cohen suggests. It may be that as with many programs well conceived originally, something is lost when it is picked up elsewhere-- I don't know; but it seemed to me that in these skills center situations those very children who (whatever our theories of language or language deprivation may be, or whatever evidence of differences in language use we may have) appear to need more help, more practice, more experience, more opportunity to interact verbally-- that these children were getting even less of an opportunity to interact verbally in this kind of a situation, either with the teacher or with other children, than they do in the inadequate normal situation.

Now I think that one of the very serious problems in most classrooms, and particularly in classrooms for underprivileged children, is that there is almost no opportunity for the pupils to participate verbally. Whatever studies we've had of classroom verbal interaction seem to indicate that, even in prestige schools with gifted children, verbal communication is initiated up to 85% by the teacher. Furthermore, the teacher's verbal product consists essentially either in giving empirical information-- facts, dates, statements of rules-- or in eliciting such empirical information as responses from the students. There is some evidence that in classrooms for the underprivileged, children are given even less opportunity to respond, even to empirical questions-- that they are frequently put into a custodial setting in which they either copy things from boards, make marks in work books or operate machines.

Now I do not mean to indicate by this that I see no utility in some of the new technology and in some of the insights into ways in which we may break down concept learning through programming materials; nor do I think we ought to ignore the possibilities of tapes and listening stations. I do think we make a mistake if we fail to look more critically at classroom interaction in order to consider, first, what kinds of changes in classroom interaction might conceivably give children more opportunity to participate verbally; and, second, whether-- if we can identify those lacks in abstraction ability which evidence seems to indicate are often characteristic of underprivileged children-- we can so organize verbal interaction in the classroom as to help children to learn the appropriate patterns.

Now as a professional educator and a lay reader of studies in language and language development, I need to continue to operate while further evidence is being developed. For the purposes of operating, I'm persuaded that language is essentially learned before children enter school. I'm essentially persuaded of the fact that lower class and middle class children probably do not differ significantly in their possession of the essential grammatical patterns in their native tongues. I'm also persuaded that apparently there is a difference in the use that lower class and middle class children make of certain kinds of patterns-- those which involve logical subordination for example-- which we assume to be critical in the development of academic skills and comprehension at a critical level. So I find myself saying: What do we teach and how do we teach it? And as a practitioner, one of the things I would have to say on the basis of what I know is that I had better try to set up problem solving situations in classrooms which will invite analytical questions and analytical evaluations on the part of both teachers and children. I wish I knew in detail how to do this. I think there are some situations where some efforts are being made in this direction, and I think this is one of the things we should work toward.

Now to move away from what we've been talking about today, I would like to speak to the question of social dialect. Here the problem seems to me a rather different one, and maybe a more difficult one in some ways. Because I suspect that this may be an area of greater sensitivity than the learning of different patterns with which we make it possible for young people to handle phenomena.

I'll say one final thing about the whole environmental setting in which I have to conceive of language and language learning taking place. In terms of the relationship of language to employment, some people who are particularly concerned with the development of subprofessional jobs have suggested that language skills, like other skills, perhaps ought to be developed on the job, rather than in the classroom, or at least on the job as well as in the classroom. I was interested to note in the report of last year's conference that both Dr. Sapon and Dr. Stewart made some comments which questioned whether or not the school and the classroom were suitable settings for language change because they were so highly structured. In both cases the suggestion was that maybe language change is more likely to take place on the street or in other unstructured situations. As an educator I'm reluctant to accept this. I'd prefer to opt for the chance to change the verbal environment as well as the social environment of the school.

Selected Excerpts from the Conference Workshop Sessions

Goals

Intervention

Techniques

Description

Language *in* Society

Goals

The question is, what's the payoff? Suppose we teach perfect grammar to all the children in Harlem. Maybe we'll find that their skin color is what causes the school to react to them in a certain way. Or has there been a study on what constitutes employability? Do we know that? In the Job Corps they just assume that it's important to get there on time in the morning. Well, what about that in relation to "She's pretty and she smiles at the boss." Yet in the Job Corps they go on teaching them to get there early in the morning all the time.

S. Alan Cohen

I sense that we are all extremely uncomfortable because we feel that we are trying to impose something we think is desirable on people who may not think it's desirable. It seems to me the answer to this is to find out whether they think it's desirable or not. My own experience indicates that by and large they do. There are various ways you can investigate this. You can present a group of people with tapes of various kinds of speakers and run an attitude study: what kinds of speakers do you trust? What kind of speaker sounds intelligent? Whom do you admire most? Does the person you admire most talk this way? Would you like to be like this person you admire? All these things have been done and the results generally indicate that what we're doing will not produce any unconscious hostility. My own experience is that it does not produce conscious hostility. Furthermore, I think we have all been operating on the principle that we're not trying to replace a dialect, but rather to provide an additional one. This is different altogether from saying, "All right, what you say is wrong--don't do it anymore."

Unidentified woman

Our task is to identify for people working in this area what I would call pay-off variables. The point is to find those aspects of language which a) are conceivably changeable, b) give us some measure of understanding and c) will lead forth into defining other variables. I think a pressing issue for this group is the question of whose job it is, and how, to make this transition from theory and empirics to practical application. There are programs which have started with theoretical and empirical givens and worked to a practical level; but there is another kind of argument that says, "Let's just get in there and try anything because anything is better than nothing."

Jeremy Finn

I don't think anyone knows enough to translate theory into practice. I don't think it should be done, in principle. In the area of second language training, for example, we've seen exactly what trivial theories can give you--trivial answers and things that evidently don't work.

Wayne O'Neil

Linguists have multiple levels of analysis and some of these might not be immediately useful. All you can expect from linguists is some information about language. It's up to the professional educator to use this information. And presumably they don't need to use all of it.

Harry Osser

We have talked at the last conference and at this one about identifying goals and about change. I think there are two questions here. One, is there enough evidence to indicate that there is a correlation between the modification of skills to produce approximately colloquial English and higher academic achievement? The other question is whether the child can apply a language system effectively according to various communicative situations. This morning Dr. Hymes brought up the term communicative competence, but if we talk in terms of communicative competence without identifying those situations which define a particular task, for example a vocational task that this child may eventually face, then I think we're going to continually deal in terms for which it is very difficult to develop research designs. I think we should address ourselves to operational definitions that we can actually test out.

For what purpose do we want to modify behavior? We haven't I think, identified the purpose adequately for research, and I have the impression that the linguists are afraid to come to grips with this issue. They want to discuss the various kinds of change, identify the parameters of the various problems that arise and then leave it there. I think there's more to this problem than that. I think the responsibility involves identifying the purpose of change. I think we have been begging the question as to whether the speech characteristics of children from socially disadvantaged homes really constitute a problem. There have been assumptions made that because socially disadvantaged children don't speak well, therefore they don't do well in school and they are not as employable as other individuals in our culture. Now I think we should be talking about what body of knowledge we can eventually accrue which will help these children, in this interim period, become adapted to the general culture.

Michael Marge

The assumption all through these discussions has been that the difficulty of the Negro child is that he has a different dialect, that this is what makes for his problems in school, and that somehow we have to change this dialect because it's not satisfactory. I think a prerequisite to teaching these children ought to be that the teachers learn the Negro dialect, because implied all the way through this discussion is the idea of improvement--the other is a deviation, it's inferior. Other countries have this problem. In Mexico if a teacher goes to teach in an Indian village he learns that dialect or that language. He teaches the children in Spanish because to continue their education the children have to know Spanish, but there's no implication that one language is inferior or that he has to give up the other language.

Carl B. Zuckerman

To say that lower class children may have a very expressive way of speaking which is interesting and effective for communication purposes within their own domain does not solve the problem. Even if you could, you are not going to transform the dominant group into all Oscar Lewises who like expressive language. That is not a feasible result. Now these children fail in English. They score low on standardized reading tests. This is what happens to them in a school situation. I don't think this necessarily means that they are incompetent in communication--depending on how you define communication competence--but it does define their unacceptibility to the school.

Marjorie Smiley

It's pretty clear I think that if you choose your tasks right, you can dissolve any social class or IQ differences you want to. Isn't it more important for those of us interested in schools and education and getting kids to move in this direction to make it quite clear that there are certain kinds of tasks we're going to use as criteria, and then to explore the conditions under which these various kinds of kids can be made to perform more effectively on those kinds of tasks? This is a different kind of question, I think. Maybe the first thing that we have to learn is that creativity and many other constructs, language fluency and communicative effectiveness, for example, are really very much task centered, and we've got to stop talking about them as general constructs and start talking about them within tasks.

Lee S. Shulman

It seems to me we've gotten to a point historically where the teaching of language has become narrower and narrower in the schools. Originally, of course, the notion English was a kind of school, but the department has narrowed until we have a whole generation of kids who know that you do this thing for English class but you never do it anywhere else. English has become a dustbin for tired ideas. What I would like to do is turn the thing the other way out so that every class becomes what it is really, a language class, the language for that particular discipline or subject matter, and every class becomes a writing class and a reading class; so that every teacher must know something about language and society, about the language of the kids she faces, about language development. Now it's nice and easy to say these things, but to develop curriculum materials is fantastically difficult.

Wayne O'Neil

Intervention

We start with the point of view that language is a vital piece of operant behavior for a human being. It is defined as a means by which an organism controls his physical and social environment with his talking apparatus rather than with his feet, and we would include body gestures and body communications in the same general pattern. Now if a child shows signs of acquiring any kind of verbal behavior it obviously has to be in some kind of a home environment (regardless of how enriched or impoverished that might be) and it obviously does have some at-the-moment-unspecified operant function. When the child moves out into the street, his operant verbal behavior is going to have to be expanded if he's going to survive in another little social subset. We've got in the planning stage a set of graded responses. For example, we get the first level of intelligibility in a child when the mother reports understanding-- ie. when she responds in some appropriate way to the kid's noise making. Another step up is when other members of the family who are in less constant contact with the youngster respond appropriately to his vocal noises. And there's another step when the child's verbal behavior can move around extra-familial adults who are extensively familiar with the verbal behavior of little children and are prepared to accept odd-ball pronunciations and highly original sentence patterns. Then we move to another level where the child's verbal behavior can control extra-familial adults who have no special preparation in appropriate responding. Thus there is a kind of crude hierarchy of expanding verbal control over his environment which provides us with an objectively determinable entering wedge. The intervention program operates in pretty much the same line except that it's a two-way street. The child cannot survive long in the world unless his own total behavior comes under effective verbal control of other people, and the hierarchy there runs pretty much the same way in the sense that it's likely to come first under the control of his most intimate parental contacts and finally to come under the control of a whole range of people. We're basing our whole attack on the evidence we have that these kinds of behaviors are teachable.

Stanley Sapon

As far as I know there's very little evidence to support any enrichment program at the moment. Now I don't think this is a calamity in itself, just a commentary on the amount of time that people have been involved in such research; and I don't think it means necessarily that if you develop enrichment programs that they will have no effect at all on children. We just don't know enough about whether certain language enrichment programs work. I don't know that anybody has brought together the information that may well be available, but it would certainly be of enormous importance to all of us.

Harry Osser

Now the transformation people say that some of these dialect differences are very superficial ones that don't matter very much. But unless we know where the discrepancies are, and unless we can specify which of these discrepancies do matter in some sense--matter in terms of acceptability in getting a job, matter in terms of understanding the teacher, or matter on some criterion, until we know more, any intervention program is very much of a shotgun business. We do something general called language enrichment and hope it pays off.

Now is it possible that the role of specifically language problems has been highly overemphasized in terms of education for disadvantaged kids? There are two questions that we have to address ourselves to. The first one is, do we have enough evidence to indicate that by modifying language behavior we're going to make these children eventually more employable? Secondly, what evidence do we have that language is related to cognition? I know we're assuming that if you help the child with his language he'll take off and go, but maybe this isn't so. It may be that problems of non-linguistic subcultural differences, whether in attitudes toward school, kinds of reinforcements, bluffing behavior, may be as important or more important than language differences to school success; and that such things as the passivity dimension which may have nothing to do with the language but everything to do with the kind of socialization that the child has undergone, may be very much related to cognitive skills and abilities. Unless one attacks these differences, maybe changing language isn't sufficient.

Courtney Cazden

What happens in some programs is this. IQ tests go up, the kids know labels ad nauseum, fine. Sure they're reading at the fourth grade level--and since they get better grades they are going to be more employable (though they still have their dialect, so in terms of getting a job they still have that problem). But they don't use language spontaneously in the way that middle class children do. They don't question the teacher, they don't question each other. They're very passive learners and these are differences I think we should be aware of. The oldest group is in the second grade now, so they haven't used reading yet for a real problem solving tool. They're not going to an encyclopedia and finding things yet, so you can't judge that effect yet. But I wonder what's going to happen, let's say, when they're in high school.

Abby Sher

Now as to the question of whether or not a particular speech code is teachable in any formal sense, as modal classrooms are set up typically, you are not asking children to perform in the kinds of spontaneous ways that would be reflected in the kinds of speech they are not spontaneously displaying. What they are demonstrating is good perception of the situational differences. I have read a note recently on a dissertation in which a group of high school juniors were interviewed in two speech situations, one a relatively academic one, and the other very unstructured. In the informal situation all of the children spoke in a Southern lower class dialect, but in the formal situation, some children spoke in what approximated standard acceptable English. The relationship appeared to be with achievement motivation. Now it seems to me that here you have differences in the situation and in the perception of the situation affecting the kind of speech performance that you find. There is a suggestion here that you don't teach standard English, you teach achievement motivation. You'd have to have at least some speakers of standard English around, but I don't know that you'd have to have a total model.

Marjorie Smiley

In any kind of language learning you've got motivational problems and you've got structural mechanical problems. As far as I'm concerned the two things always work together and always have worked together in foreign language teaching--and in that sense I see nothing different in the current situation. You can change a person's propensity to learn French, for example, partly by the kinds of motivation he has--how much he identifies with the culture, whether he feels there's a need to learn French, etc.-- but at the same time, whatever his motivation, there are certain purely linguistic problems in learning French, and by setting up a course that takes French-English differences into account and drills on the differences, you can help overcome these problems. Now there are always individuals who with the right motivation will learn very rapidly, so we've got to be very careful about generalizing our success on individuals. There are Negro kids in a ghetto situation in cities in the United States, who by themselves, on their own, with the right motivation acquire a fantastically accurate knowledge of Standard English. Other kids can't, even other kids with the same motivation. So it seems to me that our teaching materials have to be built not just on the basis of motivation, but on the basis of setting up an effective language teaching situation for the child who hasn't got this capacity of automatically internalizing new language structures for second languages later in his life.

William Stewart

We're supposed to be talking about intervention. Now intervention presupposes at least two things--one, that we know what we're intervening into, and two, that we know what we want to accomplish by the intervention. Now in Headstart last summer, the most widely reported subjective results were that the children made the greatest gains in the area of communicative and social skills. There is not one shred of systematic evidence to support this conclusion. One, we have no adequate instruments for measuring this. Two, nobody knows what to look for. Three, we don't know where we want to go. And four, we don't know where these kids are when they start. To get down to the concrete brass tacks--we need adequate descriptions. What do these kids come with? What is the entering behavior? What can they do verbally? And finally, how does this relate to where we want them to go and what we want them to do? What is it that we want these children to be able to do? Can we define the terminal behavior? I think the greatest contribution that we could possibly make at this conference would be to lay out in concrete terms what some plausible but highly specified goals might be. Maybe we don't want to change these kids. Maybe the best thing to do is just to leave them alone. They've been going on for hundreds of years like this--and the world has always been this way. But I don't think we believe that. We believe that something can and should be done. We're agreed that if the Negro is confined to the ghetto because of a million and one things, it's still going to help him somewhat if you can knock off ten of those.

Vivan Horner

I wonder whether we have underestimated the sorts of information we can acquire by actually trying out carefully selected procedures of intervention. It is quite possible that the only way you can really learn about the nature of an animal is to try to change him. Work with the disadvantaged, systematic studies of the process of change in children's language, may be the sources of really very powerful new theories, if we try to see what really fails. We ought to be looking at whatever possible intervention endeavors might be available or have been tried or ought to be tried.

Vera P. John

I wouldn't necessarily assume that we all agree that these kids need more verbal interaction. Most of my projects involve trying to teach reading, and I've been saying for some time that I don't think all language development is so crucial in beginning reading. For my purposes right now, I'm not sure what frequency of certain structures, for example, is needed for me to teach a youngster to read the basal reader in grades one and two. All through linguistics, all through the behavioral sciences, nobody is asking enough of these questions. We're just assuming certain things are important, and yet we really have so little of the type of research which would help us find out--if we define reading as the goal--how much weight this certain thing carries in relationship to that goal, to that payoff. However, I still maintain that behavior research is not as devoid of truth with little "t's" as some people take it to be. We have research from 1904, 13 separate research projects which say that kids don't learn to read by looking at the shape of the words. There's no controversy there. There are lots of things like this--basic research done by behavioral scientists--that are quite applicable. The things we're doing now don't work and are not working. Now one either goes by the seat of his pants or he says "if I'm going to make this decision I'd just as soon make it on two bits of evidence as on no evidence. There are those--and I think they could make a legitimate case--that are going to make the decision on no evidence--on the seat of their pants, but I wouldn't jump out of an airplane on that basis.

S. Alan Cohen

A lot of the success in the kind of reading program Dr. Cohen was talking about was precisely in areas where there was no behavioral conflict. They were essentially teaching skills which were new skills. He wasn't teaching on top of an already established behavioral pattern of a kind that was both deceptively like and deceptively unlike. Now I maintain that things would be much different if the children already knew, for example, a different kind of multiplication system, if they were from a culture that multiplied in a different way. Then if you tried to teach the American multiplication system to them, you'd get problems he didn't have. That is the kind of situation which we'd be dealing with in teaching standard dialect to a non-standard speaker. You see our problem with groups like the lower class Negro is precisely that they already have a linguistic system. We're not teaching on top of zero. We're teaching on top of something which is already there and there are bumps. And when you get this kind of problem then that simplistic kind of teaching procedure that Dr. Cohen is talking about is useful, but it doesn't work all alone because all sorts of complications come into the picture.

William Stewart

When you get into an area like Detroit you get a tremendous sudden influx of people who live on an island of speech and who because of housing discrimination are pretty much kept that way. They have a tremendous urge for acceptability everywhere, and they need to be employable, to make their contribution to civilization in their best way. They have competence, but their performance level gives a false impression of their competence when they face employers. Here is a senior, about to graduate. He has firm speech habits--like leaving the "s" off from the third person singular--and we have not offered him anything to help him to change. Any one of these differences alone would be all right, but when you group all of these slight differences together, you have students whose language habits, developed all these many years, will hold them back in some of the experiences they are entitled to enjoy. When I was in charge of the student newspaper, I had intelligent, brilliant students who I hoped could step into good positions, who wanted to go to college, but who would surely find their language a handicap. Our traditional grammar, the other material that we had given them had not touched their language in the least.

Now if the schools are taking the responsibility of transmitting the dominant culture, and if there is a dominant dialect in which the affairs of the country are conducted (which is the way you pinpoint what is the standard language), then we want to have something in the schools that will help these children who want to progress. Granted a change might be advantageous to a child. If you assume that, then how do you go about changing a language pattern. I think the method used in teaching people to speak foreign languages fluently offers a key to a way. Involve the whole child in repeating. And offer it to him--he's free to accept or not accept. Let the students set their own goals. Get the students to make a study. Get them to compare dialects. Get them to gather data from their classmates. Get them to hear themselves and criticize themselves. Get them to discuss whether or not they feel they would be helped if their speech were changed. We did all this on the high school level. The students set their own goals and then they had a package of tapes which involved them in repeating experiences. But the children discussed all of this first. They wanted to improve, and we did get improvement.

Ruth I. Golden

Given teachers who are not linguistically sophisticated and who have been given the injunction that what disadvantaged children need is more language, both short-term and long-term preschool programs seem to have been most effective in increasing children's vocabulary. This seems to be the easiest thing to do. Significant increases in the Peabody Picture Vocabulary Test, even in Project Headstart in summer, 1965, and similar changes in the Stanford-Binet have been found in a variety of projects throughout the country. Then comes the question of whether these vocabulary changes are relevant and useful for reading achievement. The rather systematic survey of the literature by Jeanne Chall of Harvard has shown that language is not among the skills most significantly related to early reading achievement. Anyone who thinks about it for a moment will recognize why; since the amount of language needed for first and second grade reading is clearly quite limited. The strong language training in the pre-school therefore will not "pay off" in early reading. I personally am very convinced that it will pay off in other ways, in terms of an orientation to active learning by means of drawing on verbal mediation and so forth. But our evidence here is less systematic, more promissory than final.

Vera P. John

Most of the studies we have now refer to individual words. There is some evidence to suggest that if you add to the vocabulary of children, or if children use the vocabulary that's available to them in performing certain conceptual-like tasks, their performance on these tasks is quite different from that of children who don't have the specific words in their vocabulary. The studies we have suggest that you can add to a child's vocabulary very rapidly, but I'm not quite sure what that means, because I know of some evidence to suggest that giving the child words that correspond with a number of different concepts--color names, names for shapes, names for sizes--is not adequate to develop those particular concepts in children. I think this is interesting experimentation, but I would hope that in time we'll be able to go to a linguistic level of greater complexity and look at, say, the relationships between the grammar of the child and conceptual development. What we clearly need are studies to show the value of adding to the structures available to the children. Now, if you were comparing two groups of children, I'm not so sure it would be very important educationally, so long as each group had the same structures, if there was a difference in the frequency of occurrence of structures. Say group X used structures a, b, and c 100 times in the particular sample of speech that you managed to get and in group Y you got 100 uses of structure a, but only 50 of b and c. If they have the structure that would seem to be the essential thing. Then you could focus your attention not on trying to give the structure to the child, but on trying to develop situations in which you could perhaps elicit that structure more frequently.

Harry Osser

What can we do about changing the grammatical structure that seems to be built in at a very early age. I know that I don't have to sit back, nobody has to sit back, and start thinking about how to speak--whether to use verbs here-you just take it automatically. Now there is the possibility that the rules that have been assimilated by the small group in the study that I've been involved with are only a part of the rules of Standard American English, that our youngsters are not functioning with the grammatical structure which would be expected in the normal classroom. Therefore, they may be going off linguistically in another direction than youngsters from advantaged backgrounds. If we find that a lot of our youngsters are missing that past tense morpheme or a number of other morphemes that are important, my question is still, what do we do? Perhaps if we get to them at an earlier stage, when they're much younger something can be done in this direction.

Clarence Robins

Techniques

You know we keep talking about how in lower class homes language functions in a different way. Some people have speculated that there isn't any verbal behavior in lower class homes, that these kids never get anything but instructions. But we really have no information based on anything other than mothers' retrospective reports or reports made by observers who are in the home. When I, a white middle-class investigator go into a lower-class Negro home, I am only seeing what goes on when I am there. I am an intruder and they are not going to act the way they act when I'm not there. So I'm now trying to do a non-destructive ecological type of study, to work toward getting some information about what goes on inside the home when there's nobody from the outside there. I'm bugging children. We have a little microphone transmitter about the size of a pack of chewing gum which is in the clothing of the child and moves around with him during the course of the day, picking up the child's verbal output and all that which occurs within the child's immediate range. Our experience has been that children in particular adapt out fairly quickly and that after a while you can expect that you are getting some approximation of what goes on before the introduction of the new thing. The kinds of information this sort of study can provide, may give us some guide lines on which way we move. If we know to some extent what the children come to school with, we can save a great deal of redundancy of effort. It may be that for many of the things we think we're teaching these children, we are really doing nothing except structuring the situation so that the things they already know become obvious.

Vivian Horner

One of the things that I like about the notion of communicative competence is that it may let you get at something more than just whether the sentences are longer or shorter or more or less complex. It seems to me the interesting thing about the task of, for example, having a child describe a game so that some other child can play it, is that you have a real measure of the functional effectiveness of the language. It allows you to find out what his language enables the child to do and how well he can do it-- how well can he perform in a task which requires that he communicate something to somebody else. There is some evidence that lower class children communicate to lower class children as well as upper class children communicate to upper class children. But Cherry found in her study that the lower class child had a harder time with middle class speech. It seems to me that we not only need samples of speech in a lot of situations, but in a lot of specific tasks where we can get at the functional effectiveness of kid's language.

Courtney Cazden

Piaget's tasks are clearly bound up with language. We've been trying to get through the language problems, to develop techniques where instead of asking the child "which one has more?" and so forth, you put him in a situation where you violate what would be his expectancy if, in fact, he had a concept like the conservation of continuous substance. For example, you take a 50 ml. beaker full of water and you pour it into a full liter beaker--and damned if it doesn't look as if you've filled up the entire liter beaker with this tiny little beaker of water. What you've actually got, of course, is another invisible source of water. Many kids who could not respond appropriately verbally, who could not tell you when you asked them whether there was more or the same, would indicate quite clearly by their actions that they knew something was wrong. Other kids didn't react at all. I think the research strategy is an interesting one which might carry over to other kinds of tasks. Decide what the appropriate behavior will be if you've got the concept, violate the expectancy, and see if the kid reacts with a startled response. In this case, it was quite clear that the underlying construct or structure of the conservation of continuous quantity happened before the kid could give the appropriate verbal response. Now is there a non-linguistic cognitive structure there--are we beginning to get at a meaningful distinction between a cognitive structure with and without language? I don't know.

Lee S. Shulman

Though I know that linguistic theory would support it, I don't agree with beginning children reading in their own syntax. You are reinforcing the very thing you want to teach them not to do at a later date. I think the standard forms should be there in some way or other because the transfer just does not take place later on. What we can do is to try to use the areas of greatest overlap. We should look at the core structures which the two systems have in common and build our material around those. For example, we ought to eliminate from beginning reading all those sentences which use the copula. We ought to give them the type of sentences which use the verb. Then later on, when in the classroom situation they have been learning to use the copula, then we insert it. What we see now is that after years of remedial work students come to college making errors which are basic, even though they have been taught all through elementary school, all through high school that these things shouldn't happen. If we don't start at the beginning reading stage, showing them the standard forms, they're likely to go all through life not getting any feel for them.

Beryl L. Bailey

If it's admittedly difficult to teach a child to read a linguistic system that he doesn't know; and if, on the other hand, reading is important for acquisition of a different dialect, then where are we and which can precede which other one? I certainly feel that if you want to teach reading, the material has to match his structure; the emphasis should be on comprehension, not pronunciation. One of the important things is to get the child to read a sentence. He may read it with his own non-standard pronunciation and therefore he may be judged by some types of criteria to be a bad reader, but if it makes sense to him, if he reads and understands what the words are, I think he's making progress in reading. Now the non-standard Negro dialect among kids in Washington, D.C., is quite different syntactically than standard written English. For example, where in standard English you'd say something like "my brother and my sister are playing," in the non-standard dialect you'd get something like "My brother and my sister, they are playing." The subject and the verb aren't directly linked syntactically, but instead the pronoun is always the subject and it and the nominal subject are sort of in apposition. Now if you have a straight standard English structure, the child may be able to read the sentence, but he doesn't know what it's all about. The second you change the structure just by a little bit so that it matches his own structure, by putting pronouns in there, then the whole thing is comprehended quite easily. Often he can get through rather difficult words like "thought", where the spelling doesn't match any simple sound-spelling rules he may have learned, because the context is so clear that he can make decisions rather rapidly. When you use his syntax to start with you allow him to concentrate and problem solve on areas which are important. He can look at a word that he can recognize. He can look at the spelling of it. He can do all sorts of things. If he's plodding through something that is totally meaningless to him, all he's concerned with is "reading" correctly in the sense of producing the right sound sequences and so forth. The thing is meaningless and I don't understand what he learns. It's probably something much more akin to ritual religious text reading.

William Stewart

I was raised in Brooklyn along with the tree, and I remember well that my readers and the pictures that illustrated them were the greatest thing in the world. To me a house was a big tall brick oblong with little windows in it, but there was a picture of a thing made of wood with a white picket fence and some green stuff around it. And there were animals that existed only on paper. All that talk about Daddy coming home with a briefcase under his arm-- my Daddy didn't even go to work in the morning. It was exciting. It was like science fiction to me, and I strongly suspect that if I had had in my reader a solid representation of what my life was like as a kid, I might not have been interested in reading about it. I'd had my snootful of it.

Stanley Sapon

Description

There are two things which come across in this conference. First of all there's the obvious search for the alchemist's stone. Second there's a sense of urgency. First of all you have to decide on whether language is the thing. There are too many people hanging around and being culturally deprived and underprivileged for you to take every possible thing into account, the social, the cultural, the ethnic, the subgroups and so-forth. You're going to have to put your money on one horse and try to maximize the benefit to as many children as possible. Now if you're going to make the assumption that language is a good or a bad thing, can anthropologists and linguists help us arrive at some kind of model of distance between dialects. Can you order dialects along a continuum? What sort of conceptual model would enable you to order dialect differences spatially. If there's a one step jump from each dialect to standard English you might have a cart-wheel where your hub is standard English. Or perhaps you have a linear arrangement along which dialects are somehow ordered and you can characterize each end of the line any way you want. Now the language continuum is either overlaid or crossed by another continuum called status. If you are able somehow to give us a model of dialect relationships, then we may be able to make some statements about what can be done.

Arthur McCaffrey

I think we ought to distinguish at this point between two kinds of learning. Now there is the innate biologically-determined fact that man will learn language if he's exposed to it. You simply cannot prevent it in any quick way--not even by beating him. To deprive him of learning language you have to deprive him of exposure to language. That's quite different from the kind of learning involved in, say, history. Would a child by reading a great number of history books innately derive a theory of history? So by competence we don't mean a potential to be facile with language or good in language performance; we merely mean that on the basis of innate knowledge, a child will develop a grammar of the language he's exposed to.

Wayne O'Neil

One of the problems is that you can have what appears to be a lexical identity that is functionally disparate. A very simple case in point would be the term "thank you." In some parts of the world when someone says "would you like a cup of coffee?", the reply "thank you", means that you don't want the coffee. In some parts of the world "thank you" means "Yes, indeed, I'd be most grateful." There is a lexical identify here and absolutely no way of determining the effect of what appears to be a lexical and distributional identity except by some deviant unexpected behavior, and that's not always easy to identify. Another crucial point has to do with function as opposed to response topography. It doesn't matter so much how a word is pronounced, but rather what are the purposes and what are the consequences of this verbal behavior in my presence and in the presence of others. This is a functional approach. One of the things that might happen as the consequence of a real gung-ho effort in this direction might be the achievement of a kind of dialectical leveling such as has taken place in some countries.

Stanley Sapon

There is a great tendency now for some linguists to want to soothe the feelings of middle-class Negroes who are nervous about discussing characteristically Negro behavior, since they see it associated with racial stereotypes. Partly because of this, there's been a great tendency to say that Negro dialects are really quite different from each other and differ from city to city. But what some of us have suspected all along is now being corroborated by research coming in from many cities, and that is that many of these features turn up all over--this non-copula thing, the use of "be". Now you have very little copula dropping among lower class white children, even Southern whites. When the white Appalacian child moves into a community such as Los Angeles, he is in a sense part of the dominant ethnic group. In one or two generations the child is linguistically quite assimilated to the dominant group and is a standard speaker. When you have a Negro child moving into the Los Angeles Negro community, the residential areas in which he can live are quite predetermined, therefore his dialect area is quite predetermined, the peer group he's going to have is quite predetermined. In these Negro neighborhoods the dialects get preserved for generations and generations and generations. The Negro is thus involved in a socio-linguistic situation in many ways quite different in its results from the situation of whites. Labov, working the lower class Negroes and whites found very interesting differences right across the board--which is the sort of thing you would expect when you look at the social separation.

William Stewart

It's quite clear that there is an enormous need for particular kinds of information that can be used by teachers to somehow transform these children. I know very little about Negro dialect, but I've given a couple of papers on it, and I've been inundated with requests from people throughout the country who say, for exmaple, "We are developing a Headstart Program--we want to know as much as we can about the language of disadvantaged children--we want to develop language enrichment programs for the disadvantaged--what can you do for me?" And what I reply is nothing. Now teachers are in such urgent need of curricula that they somehow misunderstand that what we lack is a description of children's speech. We know quite a lot about children of one year of age up to two years of age, and there have been a few experimental forays into three-year-old territory. But we don't know anything about children over three years of age. Studies of five-year-old children, for example--good studies in terms of psycholinguistic sophistication--don't exist, with one, two or three exceptions. There is very little known, and in order to develop curricula you simply have to do a great deal of study of children's language. Now if you describe the language of the disadvantaged as fully as you can, and if you describe the language of the advantaged as fully as you can, you may or may not find discrepancies between these two systems. If you can locate the discrepancies--presumably always, though perhaps not, in favor of the advantaged, you can develop program materials of some kind that will enable teachers to develop the linguistic resources of the deprived. The argument is that simple. But I think it will be many years before we can do that unless a very large number of people suddenly become interested in describing children's language.

Harry Osser

In the discussion of Creole languages in the Carribean, you often get the statement that the Creole is metaphoric and picturesque. This matches what I have found about non-standard dialect here. All that it turns out this means in some cases is that you have certain kinds of technical vocabulary in standard English which not all native speakers of the language know. But there is a stored capacity, dictionaries and so forth, where these terms exist. Since Creole language and dialects do not have this stored capacity, they often don't have the resources for developing technical terms. Now when you don't have a manipulative creative tradition of handling the language, you borrow from the outside. Consequently, a lot of this technical vocabulary that's in the standard language is either borrowed full hog into the Creole or the Creole describes what it is in a different way. Whereas in the Standard you've got a very technical or literary type term for something, in the Creole you often just say it with a sentence. This is interpreted as expressiveness, as being metaphorical.

William Stewart

How much sampling is necessary? I know that many people are persuaded that all you have to do is investigate a very small number of children and this may be the case. It may be that such is the nature of language that given two or three children of a certain age you can discover linguistic characteristics of children of that age. I wish I were persuaded of that. I think you can learn a great deal from a small number of children if you're a little bit lucky, but if you happen to hit upon non-verbal children, you're in trouble. And suppose you hold the position that really these two groups of children, the lower class and the middle class children are essentially similar in terms of competence, but differ only in performance, so that all you have to do is elicit from the child all of his structures. And having done this you say "well, I have found differences." The rejoinder may be, "Well, you haven't sampled adequately." I just don't have any way of answering that easily.

Harry Osser

Language in Society

Though I'm not in favor of gross transfer of foreign language techniques to the problem of second dialect, the issues are much clearer if you conceptualize the problem in terms of the bilingual situation. I think in the case of the American Negro there is a very special problem, compared to say the son of an immigrant worker who grows up in a Polish-speaking environment. We have had several waves of immigration, and in the course of a few generations these immigrants have been absorbed into the society. There are characteristic patterns. First generation the children are bilingual. Second generation they understand the foreign language but they don't speak it anymore. By the third generation the old language is lost. One of the things that has not happened to the American Negro community is that they have never been assimilated, not by generation after generation after generation. They have continued to be restricted to ghettos--and they have grown up in isolated speech communities. But the problems they face when they go out into another speech community which makes different demands upon them are really very similar to those which face, say a speaker of Spanish in Columbus, Ohio. Fortunately, three generations from now I would guess that the Puerto Ricans won't have those problems, but three generations from now, if we don't get on the stick the Negroes are still going to have the same problems.

Vivian Horner

You've got a lower-class child and he's got his language and you've got a middle-class child and he's got his language--and they may not be the same languages. What happens when that child goes to school? Are the relevancies the same for the lower class child as they are for the middle class child in the school situation? For example, what does it mean when the child comes to school, if, as studies have shown, he has the attitude that the school isn't really going to serve any function for him? I'd like to suggest that someone do a study comparing 4-year-olds who grew up in a heterogeneous lower class population--on the Lower East Side, for example, where there are many groups--with 4-year-olds who grew up in Harlem. In Harlem it doesn't matter what you do, how far up you go, you may still have to stay in Harlem.

Abby Sher

If you just look at the home and school situations of the lower class Negro child, you can come to certain conclusions about his language behavior which aren't supported when you start looking at his interactions with his peer group. For example, social workers who go into lower class Negro homes in Washington, D.C. often find that there is one-way communication between mother and child. The mothers talks to the child, but the child doesn't answer back. Having observed the child quite silent and withdrawn in the classroom, and finding the child again very silent in the home, they assume that he has got real language usage problems and that the school-room situation derives from the home situation where speech isn't elicited from him. Actually this same child is noisy as hell in a playground situation or interacting with his own peer group. A little more research turns up the fact that in lower class Negro populations the age-grading is quite severe, so that a child almost never interacts with his mother--the mother talks, but the child often doesn't talk back. This is one reason why, in Washington, D.C., you can have fairly standard speaking parents with very radically non-standard speaking children.

William Stewart

Accommodation is a two way road, it has to go both ways. The teacher has to accommodate as well as the student. A group of my students have just gone through analyzing a series of psychiatric interviews trying to find out what some of the communication problems are. There are some cases where the psychiatric social worker uses a very elaborate style. In one case they are talking about the use of obscenity and the social worker says something like, "Now what would lead you to use this particular form?" The boy answers, "You mean why do I talk like that?" In these interviews you see differences on a large number of levels, on the level of phonology, on the level of lexicon, on the level of syntax, and most importantly on the level of value systems. It turned out that this was a teacher turned social worker. She had a middle-class value system and the other person had a lower-class value system. The diagnosis that we came to was that the worker was not really getting through at all because of her failure to accommodate.

John J. Gumperz

So you've found social class language differences. What does it mean? You can find social class differences on other levels too. I think we have to put this in a broader framework--what is the relationship of language to cognition or where do language codes arise? We're working on a Bernstein hypothesis that what the children are learning in the home as native learning behavior is relevant to the school. We want to know how the child characteristically deals with his environment, how he sees objects and how he plans things--how he functions in his environment. For if you push Bernstein, you've got to account for social mobility. If lower class children have only restricted codes and middle class children have both restricted and elaborated codes, how do people in the lower class move from lower to middle class? This is something you set up before the children come to school, no matter what age they come to school. There are probably some children of lower class people who by all the criteria that we've used to determine social class are lower class deprived children. Yet when they come to school they are middle class kids. They come from Harlem, they have low incomes, delapidated housing, everything. But there's something different in these homes. If we're going to discuss intervention then let's go back to where language begins. I don't htink you can say "all right, we're going to deal with this in schools, but we're not going to deal with it out of the schools."

Abby Sher

What is the process of communication and what specifically is the relationship of language and verbal behavior to it. We know a lot of things about human nature with relation to communication. We know it's a much more sensitive process than it appears to be and one of the reasons that it doesn't show up very often as the sensitive process that it is is because things go right most of the time--apparently we're pretty good at it. But it does go wrong--we do get schizophrenic kids, we do get retarded kids. Many people think that a lot of these things have to do with the inability to relate to other people. We are social animals. We can't live by ourselves, and interacting with other people, to me involves language. Another point I'm going to make here is that verbal behavior is, by and large, always from the point of view of the child--that is it's related to things, objects, other people in relationship to himself. When a child talks about a table or a word, that doesn't exist in his crib until he already has a pretty comprehensive knowledge of things. It seems that there is good evidence that kids don't just learn words, and they don't just learn grammatical forms, they learn whole theses of things in some senses that apparently are not clear. When you learn to name and to symbol for example, you don't learn one thing, you learn about a million. You cannot interview a three-year-old kid to find out why he calls a horse a horse. The kid has no way of telling you about the characteristics which he must have in his mind, but since he can tell a horse from a cow, he must know something about these things consistently, little horses, big horses, little cows, big cows. Little kids by around age two can apparently do this with all kinds of things, so that a mass of new data has become part of them. Now it seems to me that learning is not just a process of acquiring more and more and more things and pieces but it's a learning of whole groupings of things about which no one ever seems to be very specific.

Now by and large a child gives out words and gives back things with respect to how other people are listening to him. Language as the child learns it and uses it exists in context. It does not exist in a vacuum and I don't think it should be studied in a vacuum. Language never occurs without so-called paralinguistic phenomena, ever. Just because we've artificially layered it, and selected out from it those things that we're going to call language doesn't mean that they really exist by themselves. And I don't think it's a very good research strategy with respect to the kind of problem that this conference seems to be concerned with, to examine them out of context.

There seem in this conference to be two very disparate views on whether in fact so-called disadvantaged children have language. One group of speakers only used the word disadvantaged. One group used the word deprived. One group seemed to feel that the kids came without all the tools of being able to live in society so that you had to give them skills which they didn't already have. The other group felt they already had the skills, they had a working language and you had to figure out some way to attack these skills and put them in a different context. As far as I can see almost all the people who take what I would call a field-work point of view of language, and who have actually done extensive interviewing, seem to think that these kids have an actual working vocabulary, grammar, etc., and that Negro kids in particular are terribly articulate as a group, even the most disadvantaged of them. As far as I know, they have by all our measures, all the communicative abilities. They are certainly not deprived in the sense of describing anything they have to talk about in their world and interacting with the people they have to communicate with. Furthermore, Negro kids often have a tremendously metaphoric flowery language which they use, and this may be part of how they get their kicks, as opposed to perhaps some other disadvantaged children. Among certain kinds of white lower class people you hear "shut up" consistently. You don't see this, at least with the Negro group.

Now there may very well be groups which are truly as groups deprived. The groups for example in which there are child beatings. Characteristically these come from white families out of a hill background, Kentucky and the Ozarks, who have come, for instance, to Pittsburgh. These groups are socially isolated. They have no one outside, so the kids tag along with the parents or with the mother. They never get to see any other kids, they have no peer feedback and the parents get angry with them--life is angry for many people--and they start slugging the kid. In various other groups you never take it out on the kid--you have a dog to kick.

The fact is that most kids are brought up with their experiences limited and directed by the people with whom they come in contact. These are more or less powerful people in the different groups. In a Negro group Mamma and Grandma turn out to be much more powerful in many senses than they are in most of the other American groups--in some of which Papa turns out to be extremely strong and in some of which, like in middle-class America, it turns out to be a see-saw in which everybody's on a treadmill. The child is exposed to this and is a product of it. And by and large the models that people have tend to share a lot of things in common--this is what you mean by a social class or an ethnic group--that they tend to have an awful lot of things in common. We are not all individuals in all customs--we share a great deal.

There is a pan-Jewish dialect and there is a pan-Jewish walk. Not everyone has it, but when you've got it, that's it--you are a member of this group. Now through several generations lots of people have unlearned this. But they are not easy things to unlearn because the models for unlearning them aren't generally available to people who have them. Basically these things are still pan-United States. They came from similar areas in Eastern Europe and by and large they are retained. So that I don't want this notion of commonality restricted to just one group. It's not there among many Negroes--it's not there among many of anything, but when it's there it's fairly widespread.

So each group has, in effect, all kinds of in-group and out-group things, some of which they share inside the group and some of which they share with the whole community. There's no reason to think you can't have a lot of cross-cutting things. Those of you who were in fraternities have certain things which are intra-fraternity, certain things which are intra-familial. You can have a lot of different cross-cutting social structures all operating at the same time. Now I think you can find out what the shared assumptions of various populations are, but when you talk about cultural variation within the United States, for many people this is confused with racial stereotype. If people are culturally different you either pretend they aren't or you don't look at the cultural differences and they'll go away by themselves. The thing is that until one looks at these differences I just don't think much can be done in the classroom situation--there are just too many differences some of which may influence the school situation and some of which may not, and off-hand we just don't know which do and which don't.

What we're doing in effect, then, is taking these kids into a school and saying that they are all in a way equal. Now on the face of it this is a very strange kind of proposal if it is true that they come to the school endowed with all kinds of different ways of looking at the world. For example, there are none of us who have wandered around Latin America who haven't gone through the shock of having to play kneesie with our male informants. This is a very difficult thing to get over for most of us, and in some of our cultural backgrounds apparently there's effectively no choice about things like this. These are the kinds of things kids come into school with. The notion of positive reinforcement is nice except that kids apparently get their rewards in different ways. Some kids are taught early to use verbal rewards as actual rewards, but it may turn out in some situations that you have to do a lot of touching in order to tell the kids that they're really with it--and that they have in fact succeeded. Setting up goals is not enough because you've got to set up the symbol of what success means and that it is demonstrable. A teacher may know how to do this, but he comes in with his own problems and if he comes from a good Anglo-Saxon white background, by and large he's a non-toucher.

We also have to know about old hates. If you get into a place like Pittsburgh you find that all the old Eastern European hates are still there. What you're doing in a school room is putting people together who have already been taught that the guy next to them is a no-good guy and he's out to get them. In many cases if you are sure the outside is oppressive and it doesn't show any signs, you have to do something to make the outside agress--otherwise you get very nervous. This sort of thing may well be happening in the classrooms with Negro children--and with teachers too. One of the patterns in Buffalo is to take the very good school teachers and put them in the disadvantaged schools. I happen to know personally one of the very good school teachers and this is a lady who has about as much trouble with Negroes as anyone I know--I mean major, long-term historical problems. She's a good teacher for good white middle class and Jewish kids. When they put her in a colored neighborhood, as far as I can see she's terribly disruptive. When you attract a school-teacher out of one group into another, you're dealing with very deep kinds of things. For example, certain parts of Northern Europe are sure that the child is basically a bad, sinful thing and that if you don't control him very carefully, he's going to explode. Now among the Southern European groups the assumption about the nature of kids is that they're basically good and that if you give them their way they'll be all right. So you've got two different major breaks in the assumptions of police and teachers and other people about how kids act, and by and large the kids respond to the assumptions made about them. It may very well be that certain groups just do not work well with other groups.

Now these kinds of things are the explanatory types of devices which you start worrying about when you find out that certain things don't work. I'm much more for action and learning why things don't work. This is my own bias. If linguists have learned much of anything in their wanderings around the world, one of the things they have learned to do successfully is to bring people who don't talk the dominant language into talking the dominant language within one generation. There are technical problems at all levels and some of these are language. But the general problem of disadvantage seems to me a general problem. You can't do anything about it in a technical sense. Even if you knew all the technical difficulties you might not be able to influence these because of other things. You have to have the technical things--you may have to have all kinds of materials. But failure is not always due to the materials, since the ability to implement them is a very special ability in which language isn't different from a whole bunch of other things, and no materials will work all over because you've got to deal with a series of problems.

Now if you're not smart enough to worry about which kinds of problems you're dealing with, you'll never be able to evaluate how good any program is. I was in fact suggesting earlier, that there may be some groups in which the parents say to the kid everytime he opens up his mouth "shut up" and the kid is, in effect, dumb with all adults. Maybe he'll be able to talk to his peers, but in a school situation he'll always be dumb because maybe he assumes all adults are going to tell him to shut up. He's an effective cripple as far as ever being able to work is concerned because you're going to have an awfully hard time getting him to unlearn this kind of thing as opposed to having to deal with certain kinds of syntactical problems. Or if you're dealing with the problem where the whole class blanks on a teacher it doesn't make any difference what kind of material you work with, or what kind of a teacher--pretty, ugly, man, woman, it doesn't make any difference at all. The kids are not in the same environment as the teacher. The teacher has to be adaptable and has to know about the kinds of things that may be happening to her or him. What the teacher is supposed to be doing cannot be the same in all situations. He has to know if he is being taken by the kids because what they're doing is playing a game of putting down--he has to know whether to react to a threat and which kind of threat to react to.

Now my question is are these kids deprived? I think it's quite clear they're having trouble with learning to speak and to read the standard language. But to me the only relevant question is whether they actually have the skills to learn these things as do the white middle class kids or whether they are actually lacking some of the skills with which to operate. You base a research strategy on your assumption. If we assume they don't have the skills then we're faced with one set of research tasks, because then we've got to teach them some very basic skills that our white middle-class kids are learning at ages 1, 2 and 3. If they already have the skills then we're really dealing with the communicative type problems I've been discussing, with the kind of interactional problems that have to do with space, with how you set up your classrooms, with how you train your teachers.

Now my argument in the beginning was are they different or are they really deprived. Bill Stewart's view is that they're different. I've taken the position that we reject the possibility of there being any kind of a continuum of difference between primitive and sophisticated. In fact everybody speaks a sophisticated dialect, but it turns out that the controlling population in this country, those who control publishing, printing, etc., this white Anglo-Saxon, to-a-good-extent Episcopal, somewhat Presbyterian population, speaks and writes a certain kind of dialect. So that the assumption is that even though everybody speaks a sophisticated language, if you want to succeed socially, which means be employable or whatever, it has to be sophisticated with respect to all these vested interests. So it's the controlling group which we're reacting toward and disadvantage only has meaning with respect to the controlling group. So we've got to take account of differences because the people who control the country are the people whose dialects they have to learn in order to succeed, assuming that everybody wants to live together happily and all this kind of thing.

Harvey B. Sarles

Summarization

I'm delighted to find that in the program this section is called summarization rather than summary. I've been accustomed for a long time to think that verbal behavior is not random and that if people say "summarization" and not "summary" they mean to distinguish between the two. I began to think-- what could that mean? -- and I thought perhaps it means that, beyond the usual educational paradigm that first you tell them what you're going to tell them, and then you tell them and then you tell them what you told them, a "summarization" implies that then you tell them what you told them via summaries. And I see that that's the point in our Conference at which this summarization comes, namely after each session has been separately summarized. The only other alternative that occurred to me was that summary is to summarization as visit is to visitation. Ordinary people visit with each other, but when the Middle States Association or the State Education Department, comes to a University, that's a visitation. And the difference between a visit and a visitation is the high proportion of obfuscation in the latter. And it may be that that's what a summarization is intended to do-- namely to shed some darkeness on the last few days.

We started our conference yesterday with Dr. Edmund Gordon's very moving reminder that the world around us, the real world, was agonized and contorted and pressing and crying for our help. We were asked to keep that urgency in mind and to see whether by doing what we think we should be doing, adding to knowledge, we could provide something for those who are crying out to us to come to their help in some way. All of us sometimes ask: What are we doing in this mad world? Why are we talking all the time; why are we writing all the time, reading all the time? Instead of these two days spent here at a conference why didn't we each go out and sit with a child for two days and help him in some way? That's a dilemma that we face more and more as things get worse and worse outside of these University walls and as we begin to think that the people inside of these walls have some responsibility to people outside which hasn't always been faced before now.

And this brings us to both the usual dilemma with respect to applied versus theoretical concerns, as well as to a more poignant aspect of that dilemma. The usual dilemma is that here we are, mostly professional intellectualizers and professional verbalizers and, therefore, we consider as good and real and worthwhile those problems that can be easily coped with intellectually. After all, that's what we can do. It's so much neater to work on matters that, when you think about them and you gather data on them and you process the data, the result is some substantial progress if not a complete solution of the problem. We look for the greatest parsimony--it's almost a kind of aesthetic delight--and we find real delight in intellectual parsimony.

But the world out there is not neat nor parsimonius not matter how hard you work to understand it. There are people who don't want you to understand the real world. They don't want to understand it and they don't want your understanding. Work and problems in the real world are not nearly as neat as the things we can study in our laboratories and research sites. What do we do if we recognize that we're not really equipped to work in the outside world and that it's so much messier and harder out there than it is in here?

Now at this conference some of us were embarrassed to admit, and some of us were delighted to admit, that the world out there is good for us; that good practice and good theory are really very dependent on each other. This so so first of all because good theory is "good" to some extent because it does have significance, relevance, and power in the world out there. I've come to believe more and more that applications are useful for scientific theory per se. Theory doesn't remain good very long unless it continues to face the kinds of questions that theorizers themselves don't remember to ask, that only the real world asks.

This conference was initially oriented toward the junior people in the field. As I looked at my students, many of whom are in this room today, I noticed their delight in seeing people materialize in front of them that were up until now just a book, a title, or a chapter to them, and I caught many of them looking at each other and nodding to each other and I had the impression they were saying, "I thought that's what he would say, and he did say it" or, "he said it better this time or worse this time than he had on page 56 in our readings a month ago."

But I think this conference has been of even greater value to the "junior people" than just this kind of materialization. We have not only agreed that good theory cannot be devoid of applied significance, but we have come to recognize at this conference that good theory is also, even just qua theory, that which is not merely maximally restrictive to the idealized status-free, all other things being equal circumstance. That circumstance so idealized by the natural sciences is just the beginning, the handiest beginning, the easiest beginning of theory.

However, I was reminded, so many times that I must mention it to you too, that there is a more poignant aspect to the usual theory-application dilemma that came up over and over again these last few days. And that is simply the fact that not only is the demand more urgent, but the novelty of this field is really so apparent, so patent, that we almost have two dawns coming up together, and we don't know which to watch, the urgency of the cry or the extreme immaturity and recency of the whole field. And part of that recency is due to the fact that only in the last five years have we come to recognize that the patterned significance of so much that we used to think of in linguistics and in psycholinguistics, in reading, and in education, as being no more than "the static out there," "the error variance" out there, the "free variation" out there, that we didn't have to or want to bring in to mess up our neat paradigms. We now realize that much of what we excluded is really very powerful theoretically, it can make your theories more powerful by enriching their possibilities and implications; or, to put it another way, all of that static out there really adds up to the contextualization of language. All that free-variation is patterned. There's patterned diversity of repertoire, there's patterned diversity of function. There's patterned diversity of role and role relationships, there's patterned diversity of domains. These patterns underlie our new theories whereas at one time our theories were so pure that they could not begin to cope with verbal behavior in real social contexts. That which is "purely" theoretical is only theoretical at any point in time. It is that which academic people recognize at a certain point in time as having a kind of latent structure which is satisfying and orderly to them. In the last five years, we theoreticians have begun to recognize an awful lot of additional parameters that we can get excited about, and they are closer to the real world than those we used to recognize. Our recognition at this conference that socio-linguistics and psycholinguistics, have recently been enriched by exposure to the real world, should send us home with the feeling that perhaps what we know is coming closer to making it possible for us to say comething of significance back to the real world.

Nevertheless, after going back and forth in our own minds about this, many of us are still frightened and ambivalent about our relationship to reality. To some extent I think that the competence-performance issue loomed disproportionately large the last two days because it is such a recognizably classic academic problem. When we get very upset about our competence with respect to the real world, it is always nice to come back to something that is recognizable, something we learned when we were graduate students and something our professors learned when they were graduate students even if the nomenclature was different then. There are many other such dichotomies, two-pronged considerations; langue and parole was mentioned, innate and learned was mentioned. There is aptitude and achievement, and there's genotype and phenotype, and, though no one said anything about it, there's emic and etic--and certainly no one said anything about culture and society. All of these share a similar kind of characteristic--we like to come back to comfortable dilemmas when we can't solve the real world or help it very much. For a problem in this area of language and society, in addition to the usual problems of theory and application, is that such dicussions can and do descend to meta-language. That is instead of seeking a coherent and insightful language to talk about real events and data, we begin to look for a language to use in order to talk about the language that we need in order to talk about a language that will talk about real events and data. So we begin to push back further and further until we reach a higher-higher order factor analysis that is not only removed from reality but is removed from creative theory as well.

There were two major ways in which broader issues came up during the conference, and two ways in which these more basic terms were used. I consider one of them to be a sort of scientific emic parsimony and the other a very lay emic parsimony. The scientific emic parsimony results from the fact that scientists are always looking for their theoretically preferred or abstracted summary, correlation, of what they, the scientists do, what they note, what they believe, what they find useful. And one of the two terms in each of these dichotomies such as competence vs. performance, represents that kind of parsimony. In discussing lower class behavior-through-language, there are a few considerations that seem to us to be useful in formulating and predicting what we have observed, and we call that the underlying structure and the predictive model--but that is our scientific emic structure.

On the other hand, sometimes we find that real people do that too, and that there is a preferred formulation, a preferred summary that depends upon the lay values and positions on the basis of which people out there behave too. Lower class individuals, like many individuals in any speech community, have their own ideology and their own values and their own systematic views for considering the functions of language, for "when you say it one way" and "when you say it another way." What I'd like to point out to you "junior people" here is that these two different emic structures should not be confused with each other, and neither should be overlooked of course. But neither of these emic structures is necessarily correct in the sense of really being verified or even verifiable.

There were some important major hopes for this conference: namely that there would be research clarification of the description problems and of the intervention problems in connection with language and language related behaviors in disadvantaged populations. The outcomes of this conference are more modest since they are largely hunches, interests or convictions to take home with you rather than research designs. Does it help you any to go home with the conviction that many scholars believe that they can contribute to societal goals or to societal concerns as scholars concerned with the furthering of knowledge? Does it help you any to find that they believe that many social rules and many social roles are realized through language? Does it help you to go away with the impression that many of us believe that the mere existence of a verbal repertoire--that is of a speech community in which there are multiple speech varieties at the control of most of the members so that they know when to use them and when not to, at the same time that they are simply unfamiliar with other varieties controlled by members of other co-territorial speech communities--that this fact itself does signify something about the segmentation of society and the interaction between its parts? Does it help you any to go away with the conviction that many of us believe that one can describe the verbal repertoire and the role repertoire of a speech community and that these two are usually closely correlated? If it does then you have been helped by this conference.

By and large, positive convictions were expressed in the last two days in connection with basic matters of this kind. Nevertheless it should be very clear to you that most of the speakers were saying that the basic concepts and the basic techniques for describing language performance and other related social performance are very limited or perhaps not even really formulated as yet. Most of us are at the very beginning of formulating these concepts and these techniques and therefore I think we were also saying that your help is needed and your help can make a real difference to the progress of this field. And that's a very good feeling to have. You will not be working on something that is old stuff, and that somebody else has hashed through before. If you work in this area you will be working on very new, almost still unformulated problems.

But there are lots of things that are needed before your work can be successful. For example as a sociologist just listening for a few days, I kept saying to myself and writing to myself, "a group does not equal any other group does not equal any other group does not equal any other group (no matter how much a rose is a rose is a rose). We kept saying "group", but did not really try to push in any real way on the extent to which groups are mutually exclusive, or on the extent to which all groups are not equally distant from each other. Sometimes we spoke of age groups, and sometimes of sex groups, and sometimes of occupational groups and sometimes of religious groups and sometimes of social groups and sometimes of ethnic groups; and we did not, perhaps could not at this moment, stop and think whether the language distinctions between all these groups are equally grave, or when they might be equally grave, or what it might imply if they are not equally grave in terms of the kind of work that needs to be done with the disadvantaged. What kind of inter-group differences lead to different phonologies alone, to different phonologies and lexicons alone, to different phonologies, lexicons and grammars, to different languages that exist between co-territorial peoples.

Some consideration was given at this conference to what I call planned language shift, but not enough. There was consideration given to the adoption of new, that is additional varieties into the linguistic repertoire of lower class groups--how do you plan for that--and some attention was given to expansion of current varieties--i.e. assuming that the varieties which exist are limited, how do you expand them? But there are at least several other aspects of planned language shift which were not considered.

We had discussionsof the need for good synchronic description and by good, of course, I mean contextualized synchronic descriptions, and we had some discussion of the exact specification of repertoire goals--the problem wasn't solved, but it was raised. But there was little discussion of the *ideological* association between repertoire change and social change. That is, we seemed to have recognized that if verbal repertoires change perhaps other things will and must change in the lives of individuals. But we didn't discuss how this association can be ideologized--that is how one can organize a population for both verbal and social change, how one can bring a population to see the close relationship between the two. And we certainly didn't discuss the other directional possibility, namely that as a population sees that it is *actually undergoing social change with which it identifies*, it then very easily undergoes or undertakes to undergo verbal repertoire change as well.

We did provide some perspective on planned language shift. Several of the speakers mentioned that language shift exists today in great geographic dispersion. Africa was mentioned, Paraguay was mentioned, and England was mentioned, and if they were not mentioned so could have been, Ireland, Norway, the Soviet Union, much of Asia, China and India and the Phillipines and Malaysia and Indonesia. But no one mentioned that language shift has been planned for and has been accomplished *all throughout time*, *all throughout history*, not only all throughout geography. *Every school system in history has had to teach students to read, write and speak a variety other than that of the home*. Every school system in history has had to teach the expansion of linguistic repertoires since every school system has had to teach a standard variety to pupils who arrived speaking only informal vernaculars. In some cases systems, with very difficult language repertoire tasks, have attained great intellectual success although they've had this problem to cope with. The school systems in 19th and even 20th century Germany (before the second war) had exactly that problem to cope with and so had the school system in England and so did the school system of Eastern European Jewish life have exactly that problem to cope with; and certainly classical education in China and in India had that problem to cope with; so I think we could have asked ourselves a little more, what does our problem have that theirs didn't.

Because we do have a genuine feeling that *our* problem *is* different than all of those problems that have existed in schools all throughout history. There is a lot of historical and cross-cultural experience to review and I don't know whether it helps to say that perhaps it is because we now have a large school population whose *role repertoire is so restricted*, whose *role expectations are so limited*, whose *role aspirations have been so stunted* that the *expansion of their linguistic repertoire seems futile to them as learners and it seems futile to their teachers* who just can't imagine why these people would really need to excell in those areas of the dominant configuration in which they don't function and perhaps never will function. I want to stress, moreover, that good socio-linguistic research in this area must give considerable thought to *the social process that needs to be engineered*, needs to be planned for, rather than almost exclusive attention to the linguistic engineering and the cognitive engineering about which we spoke; I doubt very much that we can really move or prepare individuals for social change unless we take the social change as seriously as we do their speech and their thought patterns.

There were many arguments at this conference. Some of them came out obliquely and some of them came out very directly. Unfortunately, I think that they were not always about matters that really have tangible consequences for research or for intervention. It is reported that therapists differ more in their theories than in their practices, i.e. that they are differentially wise but not differentially effective-- they are all about equally ineffective but they explain it differently. Professors tend to do that too. We have argued about what we should argue about, and now I ask should that trouble us? There is an old Jewish story about whether a young man should be worried about being killed if war breaks out. Either there will be a war or there won't be a war. If there won't be a war that's fine. If there is a war, either he'll be mobilized or he won't be mobilized. If he won't be mobilized why worry? But if he will be mobilized maybe he will be sent into a battle, or maybe he won't be sent into a battle. If he is not sent into a battle, why worry; but if he is sent into a battle there are still two possibilities: either he will be wounded or he won't be wounded; and it keeps going like that so that it seems to him that the chances of his being killed are very small, so why worry?

Now, something like that occurred at this conference. We did argue about whether language development can really differ significantly from one population to the next. But those who thought that "Yes, language problems can differ significantly from one population to the next", then internally disagreed as to whether the significant differences themselves could be identified. Then those who thought they could agree upon the ways in which certain populations could differ from others, then differed as to whether these differences were remediable or were not remediable. In fact, on the assumption that they could be remediated, they also began to worry about whether if they were remediable, should they be eliminated or they shouldn't be eliminated? Finally, a very small proportion of our participants worried about "how do you do it" once you've decided to do so. Now that's a long way down the scale in the progression from the theoretical to the applied, and some of us seem to have felt strongly that it was an imposition to spend two days at a conference when there was no initial agreement on many basic prior matters.

I don't think that a conference in which there is no disagreement is necessarily a good conference or a bad conference. A conference in which there is great disagreement is of course a sign that Dr. Bailey did not rig the topics and speakers in advance and have them come out as though there were greater agreement in this field than there is. I myself do not believe it was an imposition to attend a conference in which there was great disagreement on basic views. I think disagreement is very healthy for science, and particularly healthy at a conference addressed to "junior people". I think the kind of disagreement we have witnessed here during the past two days ultimately leads both scientists and "junior people" to decide that they have to do more than talk about the differences that exist between them.

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FERKAUF GRADUATE SCHOOL OF EDUCATION

YESHIVA UNIVERSITY

Summary of the Proceedings of the First Working Conference on Language Development in Disadvantaged Children

October 20 - 22, 1965

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YESHIVA UNIVERSITY CONFERENCE
on
LANGUAGE DEVELOPMENT IN DISADVANTAGED CHILDREN
OCTOBER 20 - 22, 1965

SUMMARY

The fundamental question engaging the conference concerned the nature of our language goals for disadvantaged children. The ultimate goal, it was agreed, must be to make them employable. This would, it seemed, predicate literacy in standard English, but the conferees felt that there might also be other concerns to which we should direct our efforts. We might, for instance, be concerned lest their limited language usage should constrain these children's intellectual development on the one hand, or their social mobility on the other.

One of the questions raised was the relative importance of providing disadvantaged children with a socially acceptable phonological variant. In this connection it was suggested by several conferees that one of our goals should be to "increase their repertoire," and to help them attain what Dr. Gumperz termed "flexibility" and Dr. Fishman called "a range of registers," i.e. the ability to selectively adapt to changing situations when such adaptation is necessary, rather than to try to shift whole populations bodily from one speech pattern to another.

I. The first area of research identified involves studies of the children themselves, studies designed to answer such questions as: What is a language handicapped child? What is the nature of his handicap? What is the relationship of his speech difference and/or handicap to his other behaviors?

Deploring the lack of descriptive studies of both the formal and functional uses of language among disadvantaged children, the conferees agreed on the need for extensive research in the actual speech behavior of the populations involved. Mr. Hayes urged the interdisciplinary approach to such studies, pointing out that much of the work done so far by psychologists was "linguistically naive," and that linguists, on the other hand, had been overconcerned with the formal structure of the language, leaving matters of language function to the sociologists and the psychologists. "The caveats of one discipline," he suggested, "would help the other."

What are needed are descriptive socio-linguistic studies of the type described by Dr. Fishman as encompassing "the complete behavioral and linguistic repertoire of a given natural speech community." Such studies should make use of structural analysis but should be concerned not only with how the child speaks, but to whom, when and for what purposes. Because techniques now exist for monitoring and analyzing the acoustic flow ("Research has resolved itself to the atomic level and is moving to the sub-atomic" -- Dr. Sapon) researchers should keep well in mind that language is not just what you can write in phonemic transcription -- that it includes intonation, gesture, facial expression and a variety of other linguistic and paralinguistic phenomena which are also of substantial importance.

On this point the discussion provoked something of a chicken-or-egg dichotomy of approach between the linguistically and sociologically oriented members of the discussion group. Dr. Stewart urged that formal studies come first. "How can we examine the functions of a language before we know its structure?", but the majority view was expressed by Dr. Hymes who held that while we still need to work at formally characterizing the sentences and the sequential patterns of language, we must not get carried away with the ever-increasing precision of linguistic research technology. "We must deal with what is the function of language as a whole for these children -- not just with mastery of a given variant." Dr. Gumperz' suggestion was that we begin with a series of regional studies in which the universe is defined in social terms. "There may be varieties of children speaking various dialects who have in common an attitude toward the role of language that differs from that of the school. Can we pull together and find the commonalities? We need to determine what are the norms, the social factors which operate in various communities." In this regard, it was suggested, we might profit especially from studies of the language behaviors of bilingual speakers, Indians, Puerto Ricans, Mexican-American children, who operate in more clearly defined, often competing cultures. Here we can more easily examine the roles played by the two languages within their respective cultures, and we may find within one language or the other more role variants, a wider register range, available to the children. Dr. Sapon pointed out that language was, in fact, probably less functional in life for certain populations, and to illustrate drew the analogy of the hammer, which in a primitive society might be thought of solely as a coconut cracker, while its more extensive usefulness would be recognized in a culture such as ours.

In addition to studies investigating the form and function of interpersonal language, the conferees saw the need for continuing research on the relationship between spoken language and certain other behaviors among disadvantaged children. The issue of the relationship, actual and desirable, between reading and speech, elicited some of the conference's most lively discussion. Dr. Stewart introduced the topic with the comment that "some cultures don't value reading." "Therefore," he asked, "why tie speech and reading together?" Dr. Sapon demurred. The question, he said, was not one of valuing reading as an intellectual activity, but of the value of reading as one of the weapons in the armament of skills with which the disadvantaged protect themselves, or should. The migrant laborer who can't read his contract, becomes the victim of his own ignorance. The conflict seemed to reside in two conflicting definitions of reading, namely, read-as a tool, i.e. literacy, vs. reading as a school-taught function having a value for its own sake. Dr. Fishman pointed out that formal reading in school -- recitation -- was a separate speech variant, having little to do with the way people really spoke. Reading aloud was a distortion of language which, in the case of a disadvantaged child who has little daily experience with standard speech, might seriously warp his view of language in social contexts outside of his immediate environment. This would tend to be the case even more if reading instruction were accompanied by an insistence on more standard pronunciation and grammar.

Dr. Bailey stressed that what the disadvantaged child learns to read is not all that unfamiliar. It is not a totally foreign language, but the language he has been hearing on radio and TV. "Do not denigrate reading," she insisted, "but let us separate the reading experience from the speech experience. The modification of phonology is pointless in teaching reading." Through reading, she explained, you may elaborate on language usage and improve the functionality of language, but "let the child learn to read, pronouncing the words his own way providing he knows their meanings." In this respect, it is important to have reading materials which minimize interference between the dialect and the printed material. Here we should use contrastive analysis to prepare materials which make maximal use of the area of overlap, thus minimizing the interference factor and facilitating comprehension. "Faulty word recognition does interfere with comprehension in reading -- faulty articulation does not."

Some of the conferees expressed concern at the difficulties inherent in a teaching situation which attempted to discriminate between functionally important and unimportant reading mistakes. Dr. Sapon pointed out, however, that Dr. Bailey's approach represented an attempt to bring behavior under the control of standard language in at least one modality -- the written form. In considering whether this is desirable we need to ask ourselves: if we want to modify phonology and syntax, and if we also want to teach children to read, which should come first? The evidence from foreign language teaching, Dr. Sapon noted, is that success is from the oral to the written. But there we are dealing with subjects who already have command of a language, whereas with a young child we are dealing with a whole developmental phase which has an as yet unidentified relationship to cognitive development. To interfere with uninhibited speech development at this stage may handicap a child's thinking at a later stage.

This "as yet unidentified" relationship between cognitive development and speech represents another of the areas in need of research concern. Specifically, Dr. John pointed out in her introductory statement, there is a continuing need for more studies of the use of language in mediational behavior as a way of approaching a broader understanding of the role of language in cognitive functioning. "If we concentrate on improving language in order to increase social mobility, we avoid facing another major issue which is how language functions for these children. We simply have no knowledge of how lower class children utilize language cognitively." The underlying question then becomes: What is it that minimizes a child's achievement? What qualities in his language handicap a child intellectually? For example Dr. Sapon asked, will a predominance of mands or tacts in elicited speech influence the kinds of structures available for intra-personal use?

Dr. Hymes' suggestion that we attempt some retrospective studies of youngsters who make it, who learn to use language cognitively, brought up another of the conference's recurring concerns, the methodological problems involved in all of the studies recommended. Retrospective studies, it was observed, are particularly suspect because they are dependent on that notoriously selective instrument, the human memory. However, the collection of reliable data in the present was viewed as only slightly less taking a problem. First there is the difficulty of simply recording and analyzing the needed data -- data which would ideally include not only what was said, but how it was said, to whom and under what circumstances, accompanied by what gestures, facial expressions and subvocal expressions of emotion. Robert A. Hall was quoted as having observed that nothing short of videotape recording was any longer suitable for recording a linguistic event, and the conferees agreed that not only sophisticated methods of data collection and analysis, but careful planning of situation as well would be required to produce meaningful descriptions of language behavior. In the past, Dr. Fishman pointed out, socio-linguistic studies have often focused on bilingual situations in isolated communities, not because such studies are more valid, but because they are considerably easier to carry out than is the study of dialect patterns in a complex urban community. In fact given the difficulties of observing or eliciting "natural" speech, several of the conferees expressed doubt as to the validity of speech samples on which a number of theoretical assumptions had, in the past, been based.

Dr. Bailey pointed to the difficulty involved in getting, through a white examiner, legitimate samples of Negro children's speech, and Dr. John noted that in many testing situations the elicitation of "natural" speech was rendered virtually impossible by the fact that maximum social distance prevailed between the middle-class adult investigator and the lower-class child. The training of indigenous data gatherers was seen as one solution to this difficulty, but Dr. Stewart, describing the phenomenon of age-grading and age-grouping in lower class Negro communities in Washington, D.C., noted that outsiders were sometimes more successful in establishing rapport than local residents simply because, being unclassifiable, they didn't get age-graded and were able to communicate across age-group lines. To Dr. Gumperz, this was yet another demonstration of the need for population studies which can elucidate the local structure. Mr. Al Hayes observed that such naturalistic settings were essential. "We are concerned with the children's reactions not only to standard English but to language as a whole; a study which proposed to show what children could do in experimental settings would not be acceptable since the setting itself would affect the behavior." Dr. Sapon entered another demurrer, calling for pre-structured situations. "If you simply observe the behavior of the subjects in a variety of situations, then all you know when you get through is what the subject does in situation A, B, C, and D, and presumably A^1, B^1, C^1, and D^1 too. You must structure the situation, or pre-select it. Otherwise you simply have tons of raw data. Dr. Fishman indicated that a variety of natural situations could be selected on the basis of relevant social theory so that complete dependence on experimentally structured situations at this time was neither necessary nor wise.

II. In addition to studies, already discussed, which examine the form and function of language among child populations, the panel saw a need for descriptive studies of wider focus involving an examination of the communities in which these children live, their patterns of leadership and prestige, and the models they offer to the children, adolescents and adults who live in them for various roles in society. These studies would speak to such questions as: Where do these children learn to speak? What kinds of language do they hear in their homes? In their schools? In their neighborhoods? Whom do they imitate and why?

It is generally accepted as given in sociolinguistics, Dr. Fishman pointed out, that all members of a given natural speech community control more than one variant, and that furthermore, the number of such variants will be reflected in the number of symbolically distinct role variants that exist in the same community. We need to know not only what roles are associated with the speech variants with which we are concerned, but also the range of the linguistic repertoire in these disadvantaged communities. He suggested that there may be prestige roles in these communities utilizing speech patterns which do not appear at all in the conventional investigation or in the school situation. "Perhaps," suggested Dr. Gumperz, "we should start with social situations and see what effect they have on speech behavior." We could begin our study of language patterns by using the anthropologist's knowledge of social structure to perfect Bernstein's approach. In a Norwegian study of social structure and language behavior, Gumperz discovered that groups who had a complex and "open" system of loyalties were more flexible about code switching than individuals from "closed" societies whose loyalties were all within their group -- but that the second group could switch codes under appropriate circumstances.

In spite of Dr. Sapon's quip that "We're dealing with people who would rather fight than switch," it was acknowledged that code switching goes on in lower class societies, but that we know little about the circumstances which elicit one or another variant. One of the factors that needs investigation is the power structure of these communities as perceived by the residents themselves. Another is the matter of prestige and which individuals and institutions are its bearers. Clearly, though the schools are major neighborhood institutions, they are not usually prestigious. The conferees agreed that we actually know very little about the schools and what goes on in them. We need to know how the schools deal with individuals, what methods of correction and approval the school uses. We need to know how the schools deal with individuals, what methods of correction and approval the school uses. We need to know what they are doing in connection with divergent language patterns. More specifically, we need to assess language usage in the classroom to determine how much talking, and of what kind, goes on there. In one memorable accounting, Dr. Sapon demolished the possibility of extensive one-to-one teacher pupil contact in the school situation. Given a fifty-minute class period and twenty-five children, he declared each child has a possible two minutes per class, (for a grand total of ten minutes per week per class) for verbal interaction with the teacher -- that is, if no other school business is done. Much of what the children do in class, of course, is not talk, but listen. This led the conferees to the conclusion that we need to sample teacher speech to find out what kind of language models teachers are. Since many teachers have only recently eluded their own non-standard speech patterns, Dr. Bailey noted, they often speak in a far from natural style. But it is more than teacher speech that the children hear in the classroom. They hear peer speech as well. The need for extensive study of the influence of peer speech, both in and out of the classroom, was stressed.

One reason for studying the school, in spite of the unnaturalness of some aspects of behavior in the classroom, is that it presents us with patterned behavior in a real situation in which children are necessarily involved and in which the established pattern permits controlled observation of what goes on. Hence we can more easily study not only the formal, but the functional uses of language in a given situation. The problem in reaching disadvantaged populations in school may lie not so much in the formal qualities of school language but in the functional use of language in school. There have been cultures where there was a formal academic language -- time was when one had to learn Latin in order to attend school. Indeed the fact is, said Dr. Fishman, that "all schools have always taught an artificial language. Parents, children and teachers alike understand that pupils will someday enter an adult world where this language behavior will be useful." So we need to understand the how, as well as the what, of school language usage. As an example it was suggested that we investigate the fate of the question in school. What happens to a question? Is it encouraged? Is it answered? May it lead to a sequence of questions, or is it ignored? What are the other uses of language in the classroom?

One of the areas of interest in any concern with language acquisition is that involving the concepts of modeling behavior, models and role-playing. What role models are available to children, adolescents and adults in disadvantaged societies and what are the patterns of speech and behavior which express them? What, for example, are the effects of the speech patterns of TV? What kinds of language are these children "exposed" to on TV and what does this exposure mean. Dr. Sapon here challenged what he called the "suntan" theory of education that exposure creates change. Studies have shown that disadvantaged children are exposed to as much or more TV than middle-class children yet it would appear to have little effect on their speech behavior. We need to study, Dr. Cazden suggested, the attentional factors involved. How much of what they are exposed to do they see and hear? What, in fact, is the effect of the mass media in general on the language behaviors of these populations?

Dr. Sapon called, here, for some clarification in terminology. Both language learning and language acquisition imply a teaching process, he suggested. We don't talk of the acquisition of walking behavior. Learning implies a teacher -- ergo if there is no teacher in the home and learning takes place, the teacher must be elsewhere. He disputed this notion and proferred as more acceptable, the notion of language socialization. He further suggested a need for investigation of the whole notion of models and imitation. What is involved in being a model? Does the presence of a model always imply the presence of someone who imitates? What are the social factors which lead to imitation? Why does a child imitate the dropout neighbor rather than the teacher? Why in one community are the functional illiterates or the dropouts the prestige group?

And what, in this respect, is the relative influence of the peer group and the adult population on speech behavior? What especially is the effect of the home? Reference was made to studies which demonstrate the superiority of first born children in language development -- birth order differences which obtain across SES lines -- with their strong suggestion that maximum contact with adults is of major significance in language development. Dr. Sapon took the initial position that the speech of the home was fundamental and left its indelible mark on the speech of the children, but while the validity of this position in terms of language development was relatively unquestioned, the influence of the home on choice of linguistic variant was viewed as questionable. Several conferees pointed to the innumerable instances in which children of immigrant parents grow up with unaccented American speech. Dr. Stewart asserted the major importance of peer group speech, a position supported by Dr. John in her observation that peer groups in lower-class societies become important from the time the children can walk. What is quite clear, is that more often than not, the speech of the peer group and the speech of the home are similar, and that, therefore, the effects of each are not discriminable. Dr. Stewart observed that age-grading and age-grouping as he had observed it in Washington, DC. was strong enough to make any adult an outsider in child groups, and Dr. Labov's lower

East Side study was quoted as showing that the only Negroes in his population who spoke without a dialect were those with white friends.

III. For the area of research concerned with "what to do about it all," the conferees could agree on no designation except the simple one "change". The ultimate rationale for descriptive studies of disadvantaged children and their social world is understood to be the need to change something in them, or in that world, in order to help these children function more effectively in the larger society. But where should the focus of change be? Should we "fix" the child, in Dr. Sapon's words, or should we "fix" the school, or the society, or all three? Dr. John posted an initial warning. We must exercise caution, she warned, in any approach to making a minority conform to the majority. She recounted an experience at the Tracy clinic, where deaf children are kept from signing to each other in order to encourage them in their acquisition of lip reading and vocal skills. But she noted that the children used a brief period before snacks to sign eagerly to each other, in order to communicate directly. We must be careful in trying to change speech patterns that we do not take away the communicative skills the child already has. Moreover, as Dr. Hymes commented, "No language is a complete symbolization of reality, but a reflection of a society," and it is questionable whether you can change a child's language without at the same time significantly altering his view of the world.

There are, in addition, some unanswered questions as to how much or what needs to be changed. One important area of research would involve studies aimed at determining the attitudinal reactions to various codes on the part of the larger community. There are studies demonstrating the ability of various observers to judge occupational or social status on the basis of speech alone, but we have only hypotheses as to which deviations in phonology, which lexical items in a given speech variant -- which "mistakes" -- are the most noticeable. Furthermore we do not know which of these have the most negative effects on the listener and are therefore most detrimental socially. Dr. Sapon here invoked the example of the Spanish "r" which, if properly pronounced can buy indulgence for a number of other linguistic sins in the Spanish community. In making such studies, Al Hayes observed, we need to isolate language from other behavior, even though we have earlier emphasized their integration. Otherwise we run the risk of confusing reactions to linguistic variants with overall reactions to the typical speakers of these variants.

A related area for study, the conference agreed, might be the examination of the attitudes toward their own language variant and toward language change on the part of the minority populations themselves. Dr. Bailey, noting that lower-class Negroes actively resist the acquisition of the middle-class speech behavior even though they want the benefits of middle-class economic status, and suggested that we need to know why this is so. Dr. Fishman suggested that at least one of the possible explanations which had been offered, that certain variants were associated with "masculinity," was inadequate. In Dr. Labov's Lower East Side study, for example, where "masculinity" was attributed to lower-class Negro speech, girls and homosexuals display the same language behavior, not, presumably, because it is masculine. Dr. John asked a question about the intellectual effort involved in change. Recounting an anecdote about a baby sitter who, though a dropout, had learned the entire contents of a slang dictionary, she suggested that we need to understand the kinds of motivations that lead to such intellectual effort in the area of language learning. A related area of investigation, it was suggested, would be the study of the personality correlates of versatility and resistance to/or movement toward change.

Fundamental to any discussion of change, Dr. Sapon insisted, is research in methods of management and control -- in how we modify behaviors. Otherwise, having thoroughly diagnosed the problem we may have no idea what we can do about them. We need to investigate ways of altering language behavior -- using schedules of reinforcement, for example -- rather than the more punitive methods typically used now.

Dr. Gordon asked whether language changes take place more readily in Africa where people perceive opportunities for change than in Harlem where the power structure is seen as fixed? Perhaps, Dr. Gumperz suggested, broadening the range of available social relationships will effectively broaden the range of speech patterns. We must examine the settings for learning vs. the settings for status (status being defined here as a set of rights and duties) to see where and under what circumstances language learning takes place. The conferees agreed that modifying behavior through changing situations and altering community opportunities, must be considered as one of the possibly significant ways of changing language. But we need to investigate the specific ways in which increasing role versatility may increase language versatility. Dr. John reported on a story-re-telling study which she is conducting in which many of the children make use of what can only be described as ministerial rhetoric in re-telling a story. Such behaviors suggest that there may be untapped neighborhood resources to which efforts for language change may have recourse.

Another topic for investigation is the determination of which change-processes in a society lead to the preservation or abandonment of a speech pattern. There is a vast literature, Dr. Fishman suggested, going back hundreds of years on shifts in habitual language use, i.e., on the questions of why given language variants wax and wane. It might be useful to study the literature on language maintenance and language shift and, particularly, numerous instances of planned language shifts. How successful have they been?

Are there ways in which the maintenance or decay of a language has been, is, or can be supported or inhibited by press and other public information media, by the laws and by the schools?

What can, or should be, the role of the schools in language change? Initially, it was suggested, we need an inventory of the types of intervention procedures being attempted already, and some indication of their success. What is the effect, Dr. Cazden asked, of different starting materials and of various school situations? One approach, Mr. Hayes suggested, might be to make what is known about language difference known to the children, so the effort toward change will be viewed less pejoratively. We would say to them in effect, this is what you need to know to get along in the world, but no moral judgment attaches to it. Here, the conferees agreed, one runs up against the issue of teacher attitude. As Dr. Bailey said, what do you do about the Negro teacher who has just come out of the same background as the children and is fighting it? How do we change the teachers' attitudes toward children's language? So far as teacher speech itself is concerned, we need investigations of what kind of language norms are widely tolerated and what may be the best techniques for uncovering the vastly differing norms throughout the country.

Directing its attention to teacher training, the conference saw the need to investigate teacher training and teacher recruitment programs, focusing on the attitudinal and pedagogical as well as the purely linguistic aspects of such training. It was suggested that perhaps one requirement should be courses in socio-linguistics, or at any rate, a body of courses so designed as to enable academics to communicate to teachers what they need to know about language behavior.

On the matter of teaching materials, the conferees agreed there was much left to be done. We don't know much about promoting language change, Dr. Sapon noted, but the only advances over medieval techniques are the experiences derived from foreign language teaching. On the basis of some current methods for foreign language teaching it might be argued that you don't need a description of the starting language in order to change language behavior, but the fact is that the interference factor is higher between non-standard and standard English. We need a good analysis of the starting language, and materials specially prepared to cover all the areas of maximum interference, in order to provide optimal teaching conditions. Furthermore, Dr. Stewart suggested, we need to examine all content areas for English usage. Language patterns which are corrected in the English classroom may go uncorrected in Mathematics class so that "standard" speech is made relevant to only one area. Dr. Gordon suggested that perhaps such a circumstance might be useful in teaching the child to discriminate, but Dr. Stewart countered with the warning that the young person, going out to get a job as a bookkeeper might make use of the language he has indirectly learned is acceptable in discussing accounting or arithmetic.

Perhaps, Dr. Cazden suggested, we should find out what further use, beyond occasional field trips, could be made of the outside world as part of the school's effort. The fact is, added Dr. Sapon, that the school situation is completely unrelated to the outside world. "The outside world is a non-structured situation in which you must initiate action. School is completely structured and you had better not initiate action." It may well be asked therefore, whether the school is the best place for introduction of a language variant. If not what kinds of situations may be? Dr. Stewart recounted a Liberian experience which suggested that, in one case at least, language change took place in the street while it was not taking place in the school room. He further noted that in out-of-school situations such as neighborhood centers, the "acrolect" (here defined as the most prestigeful dialect in a given linguistic community) may be heard in informal one-to-one situations without all the negative associations of school. A comfortable acrolet, he suggested, may be a more reasonable model than teacher speech. And as Dr. Bailey put it, "If phonological change is indeed important, it may be that one-to-one contact between peers is the way to achieve it, not the many to one situation which obtains in the classroom."

Conclusion

The consensus of the conference, as expressed in the foregoing discussion, was that priority be placed on broadly based analytical and descriptive studies, utilizing the interdisciplinary approach - the ethnographic and sociolinguistic as well as the psycholinguistic and purely linguistic - in order to ensure that such investigations be kept closely related to the various social groupings within a disadvantaged community.

(This report has been prepared under the supervision of Dr. Beryl L. Bailey by Joan Gussow.)